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ANTAGONISTIC MUSCLE ACTION IN VOLUNTARY FLEXION AND EXTENSION

BY RAYMOND DODGE AND E. A. BOTT

Yale and Toronto

INTRODUCTION

If the right forearm in the writing position be clasped about three inches below the elbow with the thumb and fingers of the left hand, and the right middle finger be oscillated rapidly through arcs of from twenty to thirty degrees, two outstanding processes can be detected in the musculature of the clasped arm. In the first place, there is a gradual hardening and thickening of *both* the flexor and extensor muscle groups, which persists as long as the finger is oscillated. Secondly, superimposed upon this general thickening of the antagonists, there is a rhythmic alternation of thickening and thinning, which corresponds more or less directly to the overt movements of the finger.

The exact temporal and spatial relations of these various motor and muscular changes cannot, of course, be determined by such a simple exploration. Nevertheless, this method and situation will serve to demonstrate not only the above gross changes, but certain additional phenomena. Thus one may detect that the initial thickening of both antagonistic groups is apparently either coincident with or antecedent to the oscillation; the maximum gross thickening, however, is not usually attained until a number of oscillations have occurred. Further, in the cessation of voluntary movement, when the finger motion is abruptly stopped, one can detect a certain

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sequence of phenomena. The rhythmic alternation of the antagonists ceases first, then gradually the gross thickening of both groups simultaneously subsides; and one may observe that this latter relaxation begins either immediately upon the cessation of the antagonists' action or some considerable time afterwards. That is, there are two separable processes in voluntary muscle action, one directly related to overt limb movement and one not, and these can be voluntarily dissociated to a degree in the cessation of voluntary oscillatory movements. The same type of dissociation, in the opposite time order, characterizes the commencement of voluntary oscillation.

Even such an apparently simple overt act as the oscillation of a finger seems to involve a complicated and rather baffling complex of muscle contractions, whose combination and interaction in orderly and appropriate patterns challenge inquiry.

Whether any single set of principles governs all cases of antagonistic muscle action is a question only to be decided by investigation. In the human body the conditions of such action differ widely, not merely as regards whether, or to what extent, the antagonists are under voluntary control, but also structurally as regards the form of the muscles and of their attachment. In the case, for example, of sphincters, intestinal muscle, heart, etc., the arrangement lacks bony attachments, and this circumstance is not immediately comparable either with that of the upper facial musculature, which has immobile skeletal attachments, or with those cases where the muscles and their attachments are arranged for articulated movement as in most of the musculature of the limbs.

It is with the latter type of muscle arrangement that we are at present concerned, and even of this the body presents a great variety of forms which would demand special consideration. In particular, a scientific kinesiology is rendered difficult in as much as the motion of any articulated part (which may be part of the trunk or a proximal, distal, or intermediate part of a limb) necessarily depends in a most intimate way upon the whole postural condition that serves

as a foundation for the particular action. In fact, muscle action as contributory to specific overt behavior is a complicated phenomenon dependent upon numerous processes, all more or less variable. It is essential to keep in mind this interdependence of factors when planning an experimental inquiry into the nature of muscle action.

For purposes of experimentation and exposition one may conveniently regard the bodily processes with which we were more directly concerned in voluntary movement as falling into three contiguous and interrelated divisions. Structurally these divisions are: the mobile articulated members (A); the muscular system (M) operating the former; and the neural apparatus (N) innervating this musculature.[1] In addition to being structurally connected, these parts $(N-M-A)$ are functionally united as an *action system*, so that the study of muscle action must deal with the dynamics of this system.

Theoretically, investigation of such an action system can be carried on at any one or more of the levels above mentioned, the problem being to discover the more important relationships and dependencies. This would require analysis, description, and interpretation of such spatial and temporal changes as are experimentally recordable of the respective parts when the system as a whole is in operation. Practically, the exigencies of technique will restrict our attack to strategic points accessible for such study. The more central the segment, the more will psychological inquiry be restricted to questions of temporal relationship, whereas of peripheral segments spatial as well as temporal data may be secured. To express interdivisional relationships intelligibly, the concepts one uses and the data one records concerning different factors must be in some measure comparable. While ideally one might wish to portray the whole of a biological action system in strictly comparable terms, it must remain an empirical question how far this is possible or feasible.

The schema here suggested as an aid in the exploration

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[1] A fourth division, *viz.*, the media for exteroceptive or interoceptive stimulation of the neural apparatus, might also be included, but for our present purpose this division and its action is taken for granted.

of an action system is not free from difficulty. It is presented in the hope of clarifying the problems and the terminology. One advantage to be derived from this divisional conception will be the necessity of clearly stating one's problems and experimental findings in terms of occurrences recordable at one or more of the levels—neural, muscular, motor. This should restrain one, for instance, from stating his problems too freely and hypothetically in terms of activities of that level which is least well understood and least accessible for direct study, viz., the neural. Hypotheses of neural activity as governing voluntary action may, of course, be suggestive, but for psychological purposes they must be evaluated according as they furnish clues and incentives to actual experimentation with the intact action system. On the other hand, a danger from our schema lies in its over-simplification of the complexity of the action system, which involves not merely its own immediate divisions, but those of other action systems within the organism and perhaps outside of it.

In proceeding to consider how muscle action ties in with an *N-M-A* system, passing reference may be made to certain differences in the divisions we are distinguishing:

1. The mobile member (*A*) articulates upon a relatively stabilized unit (*B*), usually itself articulated, and *A*'s overt oscillatory movement presents many problems for analysis. The type of articulation (hinge, ball, etc.) and the direction of movement (in respect of other parts of the body) are probably best designated by the accepted anatomical terms, although these are not free from ambiguity, especially when function as distinct from mere position is concerned. The amplitude, rate, strength, and accuracy of displacement under various conditions of reciprocating, non-reciprocating, and restricted movement offer a wide field of study. The details of spatial and temporal relations in overt movement may be recorded by suitable means of registration, although adequate control of the factors that condition such action voluntarily on the part of the subject is often baffling. Under conditions of external resistance or load there may, of course, be little or no overt displacement of the segment, in which case muscle

pull is nevertheless reflected in some appropriate postural adjustment or pressure involving both the proximal unit B and other parts of the body. Moreover, even without external resistance, postural accommodation may accompany overt performance and affect it in different ways, depending upon whether such accommodation is wholly ignored or is experimentally controlled, volitionally or by mechanical supports.

2. Action of the muscular segment (M) is more difficult to describe than that of the mobile unit which it operates, in as much as a relatively simple overt movement may represent the combined action of several antagonistic muscles or groups of muscles. Our immediate problem (as suggested by our opening observations) being to ascertain certain temporal relations which characterize muscular contraction and relaxation, we are faced with the question of ways and means. Mechanically, action of muscle depends on its shortening and lengthening, which processes usually involve hardening and softening or thickening and thinning of the respective muscle bodies; these latter changes may be used with certain precautions as indicators of the time of muscle action. Neither hardening nor thickening is necessarily proportional to the amount (extent, strength, or duration) of muscle pull, and their reliability as action indicators may be further qualified by (a) the particular arrangement of fibres in different types of muscle—cylindrical, pinnaform, etc., (b) the tissue environs which physically encompass the muscle, (c) the progression of contraction in muscle fibre and the order of innervation of fibres in modulated muscle contraction, (d) adjustment between amount of muscle pull and degree of elongation at which the muscle is required to work, (e) variations in leverage and tautness incidental to particular forms of muscle attachment, (f) elasticity and stretch. The time registration of the action of muscle tissue is thus far from simple.

3. The neural apparatus (N) is probably too complex to be profitably considered without subdivision. Whatever phase is emphasized for purposes of experimentation or of

theory, the selection will scarcely encompass all of the neural factors that are actually involved. One's selection for this division will naturally be some working compromise between that over-simplification which ignores pertinent facts and that complication which leads to confusion or mere speculation. One may emphasize: (a) innervation of the antagonistic muscles directly attached to the mobile segment; (b) innervation of these together with that of the muscles which stabilize the frame proximally to permit articulation of the mobile segment; (c) specific receptor and adjustor processes, as well as some or all of these efferent ones; (d) in the case of reciprocal voluntary movement, consideration of the neural process as a circular one, the motion of the mobile segment being in some degree self-regulating through introceptive stimulation; (e) the assumption that the nervous system works 'as a whole'; (f) instead of such structural distinctions as the above, emphasis of functional conceptions, such as inhibition, facilitation, and refractory period. Various interests may influence the selection of what shall constitute in any instance the neural problem. For the experimentalist studying voluntary muscle action, the following criteria may be helpful in selecting this segment: (a) the degree to which the neural phenomena one accepts can be clearly related to demonstrable facts of muscle action, and (b) whether the dependence thus postulated can itself be subjected to experimental verification under the conditions essential for voluntary movement.

In the human subject, to which one is confined for experimental investigation of voluntary action, only the mobile and muscular parts of the action system can be directly examined, the neural segment being approachable mainly by inference based upon principles characteristic of physiological methods. Under psychological conditions some of the properties of overt reciprocatory movement as dependent upon the variables of amplitude, frequency, and load, are known, and some have been investigated by us,[2] as has also the effect of

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[2] Bott, E. A., 'Some Characteristics of Voluntary Wrist Action,' *Brit. J. Psychol.*, 1923, **14**, 1.

the length of the work period [3] and of ingestion of alcohol.[4] On the other hand, from this point of view relatively little is known concerning the action of the muscular system, and still less about the neural apparatus.

Accepting that in voluntary action all three divisions manifest spatial or temporal changes, experimentally it is not feasible (nor is it necessary) to portray *all* of such changes in order to obtain a fair degree of insight regarding the more important relations that characterize and condition the action system. It is essential, however, to register significant changes at appropriate levels. The selection of the relationships to be explored, the technique for their investigation, and the mode of representing the results and their interdependence are all pertinent questions. Organization of the findings we believe to be peculiarly important. A systematic plan that will adequately present the spatial and temporal data we shall term a *pattern* of the process in question. In this sense a pattern should aim to express in some standardized way (verbally or graphically) uniformities of relationship, empirically verifiable, of the system. Within certain limits individual differences in patterns may be expected, but it will be of interest to ascertain whether there are general patterns characteristic of voluntary action.

Patterns of an action system may pertain to any of the divisions or any combination of them. Thus, any significant uniformity that can be shown to be characteristic of action by the mobile member would be an overt *movement* pattern; temporal uniformities in the action of muscle groups would be a *muscular* pattern; of the innervation process, a *neural* pattern. Uniformities obtaining between overt movement and muscle action associated therewith would be a *movement-muscular* pattern; between the innervation process and muscle action, a *neuro-muscular* pattern. One is familiar with that approach to behavior study which deliberately short-circuits these problems by accepting a dynamic relationship between a gross stimulus situation and a more or less

[3] Dodge, R., 'Laws of Relative Fatigue,' PSYCHOL. REV., 1917, 24, 89.
[4] Dodge and Benedict, 'Psychological Effects of Alcohol,' 1915.

gross overt response, with all of the intermediary factors and variables of the action system taken for granted. While this level of analysis may have a certain practical sanction for practical purposes, and may be considered scientifically important for the general description of behavior, we believe that both scientific and practical interests require fuller and more intimate knowledge of the nerve-muscle-limb process than can be secured by such short cuts.

TECHNICAL CONSIDERATIONS

A review of the physiological literature concerning the action of antagonistic muscles is available in a monograph by Tilney and Pike,[5] to which the psychologist who is interested in the question should refer. For the sake of space we shall take that review for granted. In contrast to the usual physiological procedure, the question of the participation of antagonists in the motor patterns of voluntary action must be answered chiefly by experimentation on intact human subjects. As far as this is true it seems to preclude recording techniques that depend on attachments to excised muscle except in occasional surgical cases and to restrict experimentation to relatively indirect indicators of muscle action.[6]

In addition to movement of the articulated limb the possible indicators of muscular action in the intact human subject include muscle sounds, the electrical currents of action, and muscle hardening and thickening. Each of these has its own technical advantages, difficulties, and probable limitations. /Of these indicators the one which we know best is muscle hardening or thickening recorded by levers. The chief advantages and limitations, together with the main sources of error and distortion, which attend the use of muscle thickening have been pointed out by Dodge [7] and by Dodge and Benedict.[8] This indicator seems adequate

[5] *Arch. Neur. & Psychiat.* 1925, 13, 289–334.

[6] Putti, *Brit. Med. J.*, June, 1918, p. 635. Also *Lancet*, June, 1918, p. 791.

[7] Dodge, R., 'An Exploration of a Normal Knee Jerk,' *Zsch. f. allg. Physiol.*, 1910, 12, 1, 2.

[8] Dodge, R., and Benedict, F. G., 'Psychological Effects of Alcohol,' *Publ. Carnegie Instit. Wash.*, No. 232.

both for purposes of exploration and for the development of more precise problems.

The older and more widely used method of recording muscle thickening by air transmission through rubber tubes connecting Marey tambours has disadvantages consequent to the great elasticity of the transmitting medium. Particularly where, as in our case, it seemed desirable to record the onset and sequence of muscle contraction waves, the delay, over-throw, and elastic oscillation of air transmission would be an obvious disadvantage.

In view of these various considerations we relied chiefly in the present study on recording muscle hardening and thickening by light levers or lever systems, pressed against the bare skin or light clothing over the appropriate muscle bodies of human subjects. The limb segment proximal to the mobile part was always mechanically supported and stabilized. In some cases the mobile part was free to move; in others it was mechanically prevented from moving by appropriate stops. In all cases it was so supported that movement of a few degrees left the proximal member as far as possible undisturbed. During the recording the subjects usually held a convenient sitting posture with the feet on the floor, but in flexion and extension of the foot they stood on the left leg, while the right leg was flexed at 90° to the thigh and supported by an adjustable rest.

The position of the stabilized limb which included the muscles to be investigated was determined by two con-siderations. The first was an accident dependent on our vertical kymograph, which required that the recording levers move in vertical planes, and, consequently, that each muscle whose thickening was to be recorded should lie in a horizontal position. The second condition was the reasonable comfort of the subject, which required as little unnatural contortion as possible.

The consequent records of muscular action will be most easily understood if they are regarded as made by recording levers, 30 inches long, pressed against appropriate antagonistic muscles on opposite sides of the stabilized limb. Contraction

of both muscles moved the recording points apart. Relaxation of both muscles caused the recording points to approach each other.

The Recording Levers

The main considerations in lever recorders are their weight, length, magnification, and point of attachment. Data for most of these factors have been developed for tambour levers by physiologists, but the situation is very different when the recording levers rest on a blanket of skin and connective tissue over relatively solid muscle, instead of on an air cushion. The difference is still greater when one considers that the force of muscle thickening is of a very different order from that of air waves in a pneumatic system. The only data known to us concerning the effects of lever weight on the curves of muscle thickening were recorded in Dodge's 'Exploration of a Normal Knee-Jerk.' [9] He found that a weight of five kilograms applied to the muscle lever at its point of contact with the muscle had no effect on the knee-jerk latency as recorded by quadriceps' thickening, while weights of ten to fifteen kilograms apparently somewhat reduced it. The causes for this shortening of the latency need not now concern us. The main point is that moderate changes of pressure against the muscle have no appreciable effect on latency of contraction.

The effect of lever weight on the form of the thickening curve, on the contrary, was quite marked. It changed the slope of the ascending curve, increased its amplitude, and increased the elastic vibrations of the recording system. In view of these known distortions of the curve by the weight of the levers it seemed desirable to make our lever systems as light as practicable. They were made of thin strips of white pine, tapering from 7/8 in. by 3/8 in. at the axis of rotation to 3/16 in. by 1/16 in. at the recording end.

Our recording levers were 30 inches long for both groups of arm muscles and 15 inches long for the muscles of the leg and thigh. The latter were compound levers. In both cases the contact with the muscle was spaced to give a

[9] *Op. cit.*

magnification of muscle thickening between seven and eight times. Aside from magnification of the records the main advantage of long levers is decreased distortion of the records as the radius of curvature increases. This is especially useful when one desires to know the mutual time relationship between excursions which were often at quite different distances from their respective base lines. Careful analysis of the curves would of course distinguish between the muscle factor and the circular distortion due to the lever radius, but it seemed expedient in this exploratory study to make the records themselves represent the facts as faithfully as possible. Doubtless no rotating lever is a perfect device. Ideal recording probably demands the rectilinear displacement of the record lines on a plane surface, produced by dead-beat recorders with a latency below the unit of measurement. This is probably most nearly approximated by photographic records, but in a study of muscle thickening the satisfactory mounting of recording mirrors over antagonistic muscles seemed to offer many technical difficulties.

As Dodge has shown,[10] the point along the muscle axis, against which a given recording lever presses, determines the amplitude of the record and may also modify the apparent time of reflex contraction by several thousandths of a second. Since neither time intervals of this size nor absolute amplitude of the curves seemed significant in this preliminary study, we merely chose for lever contact that point of the flexor or extensor group that seemed to thicken most in the corresponding movements of, or efforts to move, the mobile member under existing circumstances of limb position. This point was found empirically by palpation or by the actual movement of the recording levers.

We tried marking an appropriate spot on the skin with ink or surgeon's plaster, hoping to make the records more nearly comparable from day to day. This device was only partially successful, since comparability depended more on the precision with which the torsion of the underlying parts was reproduced than on the exactitude of position of the longitudinal axis of

[10] *Op. cit.*, Fig. 5, p. 17, and Figs. 25 and 26.

the muscle. On the whole the empirical control of position seemed adequate for our purposes.

Three forms of contact were tried between the recording levers and the extensors and the flexors of the finger. They may be described as follows: In the first the levers rested directly across the muscles of the forearm, while in the second they rested on light strips running lengthwise of the muscle. Both of these were faulty in their selectivity, so we finally came to use a thimble-like offset from the recording lever, resting on the body of the muscle at what was apparently the point of greatest thickening. This type of contact was used exclusively in all the records, except the earliest, of finger movements.

RECORDS OF THE CONTRACTION AND RELAXATION OF ANTAGONISTIC MUSCLES

Our main question concerns the action of antagonistic muscles during the voluntary extension and flexion of a limb. More precisely, we sought to record the gross time relations of the correlated thickening and thinning of antagonistic muscles or muscle groups.

Records were obtained under varying conditions from four sets of muscles—namely, the flexors and extensors of the digits; the biceps and triceps brachii, flexing and extending the forearm at the elbow; the tibialis and gastrocnemius, flexing and extending the foot at the ankle; and the quadriceps and ham-string muscles, flexing and extending the foreleg at the knee.

The limitation of exploration to these groups has no other ground than their relative accessibility and the convenience of applying our recording levers to them. These reasons also explain why the flexors and extensors of the digits were subjected to the most extensive study.

Naturally one must be cautious about generalizing from these few groups to other antagonistic muscles, but there appear certain rather clear uniformities and differences which suggest regularity and seem to justify both the present report and further study if better technical facilities and controls can be devised.

We recorded the action of the digital flexors and extensors under four main forms of control, which may be enumerated as follows: (*A*) Oscillation of the right middle finger starting from a resting position at approximately 45° to the back of the hand. The oscillatory movement was varied intentionally in amplitude and in frequency, beginning with either extension or flexion. (*B*) Oscillation between stops, which limited overt movement to an amplitude of approximately 10°. (*C*) Oscillation from a condition of moderate pressure against stops, beginning with either extension or flexion. (*D*) Oscillation, starting from a natural resting position, between mechanical stops which as far as practicable prevented all overt movement. We have records of antagonistic muscle action taken under the conditions (*A*) to (*D*) with the following experimental variations.

Condition *A*.

> Maximum speed of oscillation beginning with flexion and with extension; slow oscillation beginning with flexion and with extension; slow oscillation beginning with flexion from a position of extension and with extension from a position of maximal flexion; oscillation beginning with flexion from extension against a stop, and with extension from flexion against a stop; rapid and slow oscillation against the tension of elastic rubber bands.

Condition *B*.

> Rapid and slow oscillation between stops; slow flexion to a stop; slow extension to a stop.

Condition *C*.

> Flexion after protracted extension; extension after protracted flexion; intermittent flexion against a stop; intermittent extension against a stop.

Condition *D*.

> Isometric flexion strain and isometric extension strain from relaxation; intermittent extension and flexion strains, rapid and slow; sudden relaxation after contraction; intermittent flexion strains, and extension strains.

Records from the other groups of muscles do not represent all these exploratory variations, but only a selected list of those that seemed likely to be most significant. Four subjects served in various parts of the experiments, namely: E. A. Bott, Raymond Dodge, F. H. Pike, and C. F. Scofield. We are especially indebted to Dr. Pike who gave us invaluable criticism and counsel.

A detailed description of the whole series of records under the various conditions of control would be very long. While their theoretical implications might seem to justify complete publication, this is contra-indicated by our hope that techniques which are now under development may in time furnish considerably more accurate records of the time relations of muscular contraction and relaxation. Our present series of records is therefore announced as filed at the Institute. Certain of the records, however, seem to show sufficiently clear and interesting tendencies to justify immediate publication. They are accordingly herewith presented.

1. Muscular Consequences of the Passive Movements of a Limb

If the leg of a subject is passively extended at the knee from a position of semi-flexion (subject seated) while it is supported by a sling whose axis is in line with the knee-joint, the quadriceps and ham-string muscles produce records as though the former had relaxed and the latter had thickened. Passive flexion of the leg produces the reverse effect. A typical record is reproduced in Fig. 1.

Fig. 1. Passive extension

Quadriceps

Ham-string
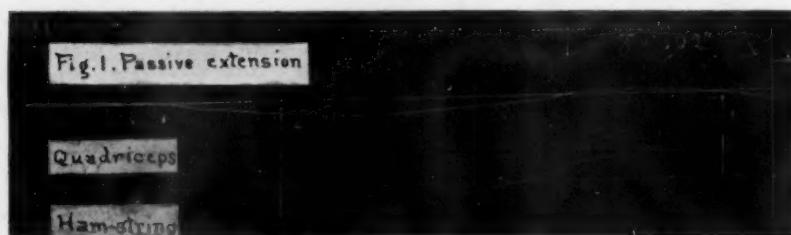
Fig. 1

The excursions in Fig. 1 probably do not represent true thickening and thinning of the respective muscles, but rather a mechanical disturbance of the elastic equilibrium between muscle tissue and the pressure of the recording lever.

The evidence for this conclusion lies partly in the mechanics of the muscle-lever systems and partly in the character of the records. When the fore-leg of a sitting subject is mechanically supported at an angle of 90° to the thigh, with the muscles of the quadriceps ham-string groups voluntarily relaxed, there remains a relatively constant tonicity, which opposes the pressure of a lever and prevents the latter from sinking into the body of the muscle beyond a certain depth. The lever and muscular systems come to an elastic equilibrium with the muscle somewhat indented. Provided the tonicity of the muscle does not change, any longitudinal pull on the indented muscle should theoretically tend to stretch the deformed fibres with a consequent partial elimination of the indentation made by the pressure of the recording lever. Conversely, any decrease in the longitudinal pull at a given tonicity should render the fibres less taut and permit greater indentation by the lever; that is to say, purely mechanical increase or decrease of muscle tension would theoretically proportionately tend to displace the position of elastic equilibrium between the tonus of muscle and the pressure of recording levers.

The records themselves support the view that these curves of passive movements are the result of mechanical changes in the muscles rather than of contraction. In the first place, the curves lack the sharp onset and sudden rise that characterize all reflexes and voluntary contractions which are not intentionally made slowly. Furthermore, the smooth and gradual curves seem to preclude the interplay of proprioceptor reflexes and lack the peculiar secondary waves found in voluntary contractions with the same recording devices. They also show homologous and contemporaneous changes in directions just the opposite from those that would be produced in voluntary movement of the mobile member. These considerations emphasize the technical advantages of taking

records of muscle thickening while the mobile member is mechanically prevented from moving. Overt movement would tend to introduce a disturbing mechanical effect into the records of antagonistic muscle, simulating co-contraction. Similarly, whenever limb movement is permitted, excursions of the muscle record which appear to indicate simultaneous thickening of the antagonists when a muscle or muscle group is voluntarily contracted must be regarded with suspicion. They may be wholly or in part mechanical artifacts. Conversely, records of overt movement which show apparent coördinate thinning of an antagonistic muscle probably understate the relaxation.

2. The Reflex Relaxation of Antagonistic Muscles

Evidence for a reflex relaxation of antagonistic muscles coördinated with the reflex contraction of a prime mover [11] is furnished by our records of the action of the ham-string muscles during the knee-jerk. Typical records from subjects Dodge and Pike are reproduced in Fig. 2 (*a*) and (*b*). In
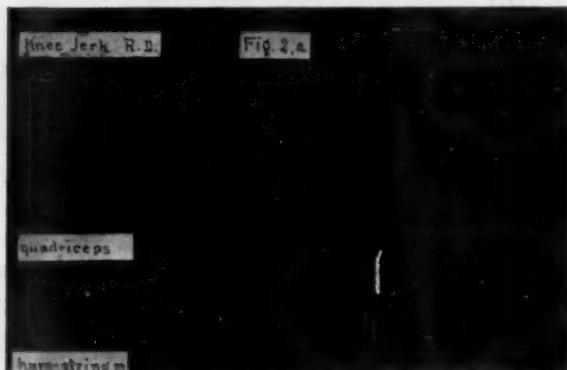
Fig. 2*a*

these records the ham-string muscles show the mechanical effects of the blow on the patellar tendon, not in a single minute twitch as the quadriceps show them, but in a more

[11] By prime mover in this connection we would designate the muscle or muscle group whose contraction is primarily responsible for the movement of a mobile member.

complex vibration record as one might expect from the mechanics of its transmission through the skeleton and soft tissues. Out of this preliminary mechanical vibration there arises, when the leg is mechanically immobilized, an un-
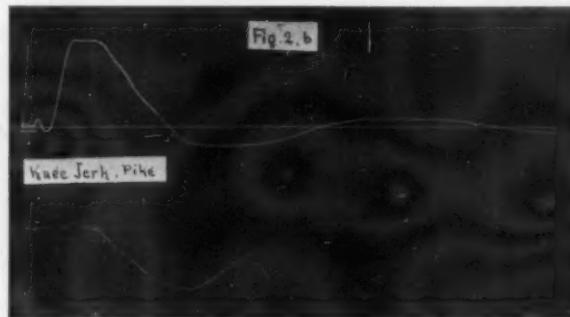
Fig. 2b

ambiguous relaxation of the ham-string muscles, which is coincident within a few thousandths of a second with the thickening of the quadriceps. At or about the moment of maximum thickening of the quadriceps the relaxation of the ham-string muscles reverses into a thickening. The details of this reflex relaxation vary in the two subjects from whom we have records just as conspicuously as the details of the quadriceps contraction itself. But for each subject both the thickening and the thinning seem to follow patterns which are quite constant and characteristic of the individual. The form of Dodge's quadriceps thickening in the knee-jerk appears not to have changed significantly in seventeen years.[12] One conjectures some connection between these primitive muscular patterns and the individual differences of gait and related mannerisms of overt movement.

A comparison of the latency of the two processes seems to furnish evidence that the relaxation of the ham-string muscles is conditioned by reflex centers at about the same neural level as those that condition the knee-jerk. It is now generally recognized that the human knee-jerk is a reflex of

[12] Dodge, R., 'Exploration of a Normal Knee-jerk,' *Zsch. f. allg. Physiol.*, 1910, **12**, 1–58.

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the lumbar cord.   As recorded from the muscle by levers it
has a latency of between 28σ and 45σ.   The similar latency of
coincident relaxation of the ham-string muscles seems to
indicate a corresponding origin of reflex relaxation.   As far
as it goes this inference from the latent time tends to increase
the probability that reflex reciprocal inhibition in man is a
phenomenon of primitive neural organization.   If we were
still at liberty to assume such a process, it would appear like
a good illustration of inhibition by drainage.   Our later
discussion of the bearing of our data on the theory of reciprocal
relaxation will suggest another explanation.   The subsequent
contraction of the antagonistic muscle cannot be so readily
placed in the neural scheme on the basis of experiment on
intact human subjects.   It is not impossible, however, that
it is a consequence of the action of delay paths as described
by Forbes.[13]

Certain biological advantages of reflex relaxation of
antagonistic muscles followed by their contraction are fairly
clear if one regards the knee-jerk as a primitive reaction of
low latency and in a commonly useful direction, but without
fine adaptation to the stimulating situation—a kind of
protopathic reaction.[14]   Subsequent epicritic control of the
movement of the mobile member might be produced through
the action of antagonistic muscle as well as through the
relaxation of the prime mover.

In view of the later discussion it would seem to be worth
considering whether or not the contraction of the ham-string
muscles is automatically conditioned by the spread of the
neural excitations that innervate the prime mover as they
affect a relaxed antagonistic system.

3. Co-contraction of Antagonistic Muscles

In our records co-contraction of antagonistic muscles is a
common consequence of the effort either to flex or extend
mechanically fixated mobile members when the corresponding

[13] *Amer. J. Physiol.*, 1921, **56**, 273–312; *Physiol. Revs.*, 1922, **2**, 361–306.
[14] Dodge, R., 'Adequacy of Reflex Compensatory Eye-movements,' *J. Exper.
Psychol.*, 1923, **6**, 181.

muscles were previously relaxed. It occurred regularly in all subjects in the following cases: in the effort to extend and flex the middle finger, to extend and flex the foot at the ankle, to extend the arm at the elbow, and to flex the leg at the knee. Fig. 3 (*a*) reproduces a record of finger oscillations with
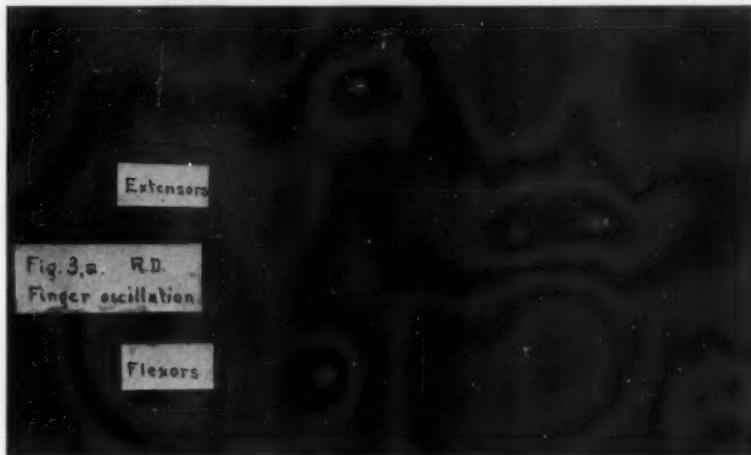
Fig. 3*a*

protracted co-contraction. Fig. 3 (*b*) reproduces a record of voluntary flexion of the leg at the knee with simultaneous co-contraction and subsequent co-relaxation of the quadriceps. Extension of the leg at the knee, on the contrary, as shown in Fig. 3 (*c*) commonly produced co-relaxation of the hamstring muscle. For both Fig. 3 (*b*) and Fig. 3 (*c*) the mobile member (the leg) was supported by a sling attached under the instep, but was prevented from moving by stops. In view of the precautions we believe that these records of co-contraction and the rarer co-relaxation of antagonistic muscles represent actual thickening and thinning of the respective muscles.

One difficulty in technique deserves mention. In taking the record reproduced as Fig. 3 (*a*), the mobile member was permitted to move, and our technique did not provide for exclusion of muscle groups that control the wrist. It is

consequently probable that this record is complicated by the
coördinate muscular fixation of the wrist.   Such a muscular
pattern would be useful in the development of effective finger
pressure.   Indeed, the latter would be impossible if the
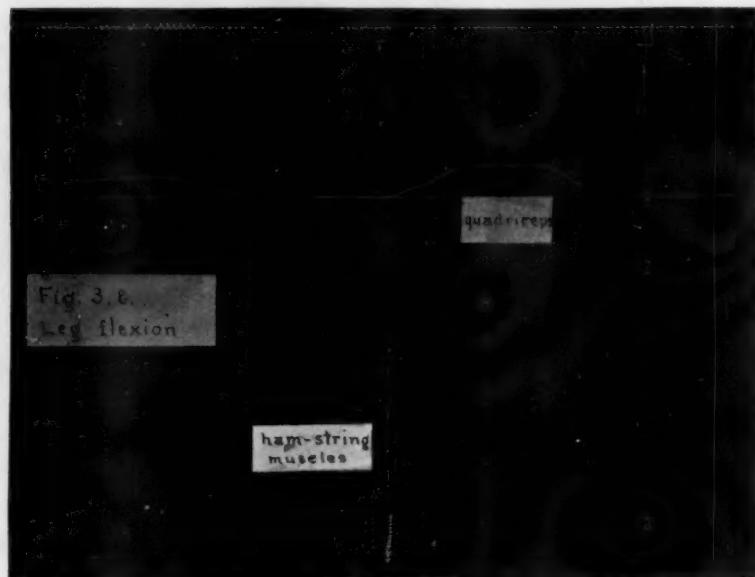
FIG. 3*b*

wrist gave way whenever the finger extended or flexed against
external resistance.   The behavior of the wrist during rapid
finger oscillation indicates that such muscular fixation prob-
ably occurred during this record.

Even the expedient of immobilizing the wrist mechanically
by strapping it to a board did not necessarily eliminate its
muscular fixation.   The presence of the latter, however,
thereby became more difficult to prove.

Taken by itself, consequently, our evidence for co-con-
traction of digital antagonists during rapid oscillation of the
fingers would be rather inconclusive.   Our records, however,
do show at the beginning of a voluntary effort to oscillate the
fingers from a precondition of relaxation the existence of a
more or less widespread muscular innervation that includes
antagonistic muscle groups.

Evidence for the co-contraction of specific antagonists is more convincing in muscle systems that permitted more selective records of the relevant muscles, as in records of the gastrocnemius-tibialis group, Fig. 3 (*d*), the extensors and
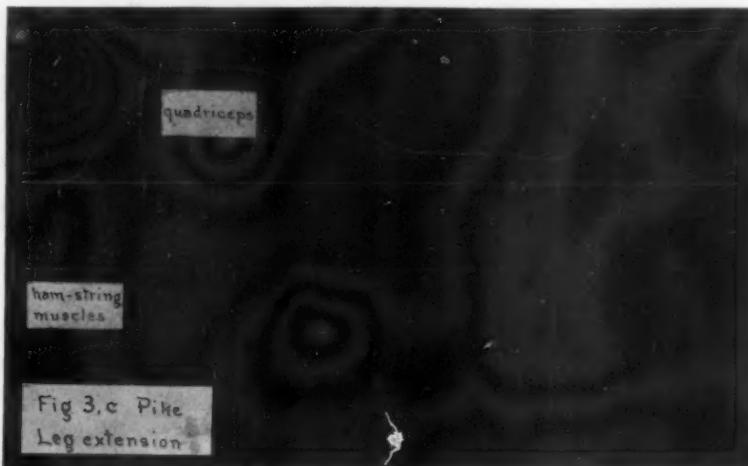
Fig. 3*c*

flexors of the ankle. While records of these muscles are not uninfluenced by the action of their neighbors, they are either preponderant by reason of the size of the relevant muscle or, as in the case of the tibialis anterior, more easily differentiated by the suitable placement of the contact thimble.

Fig. 3*d*

Voluntary effort either to flex or extend a mechanically immobilized foot from preliminary relaxation regularly resulted in a co-contraction of the antagonistic muscles in all our subjects. The records of co-contraction in this case cannot be explained by a presumptive synergic muscular fixation of the knee, since the muscles adapted to that function are in the thigh. Synergic action of the toe muscles would often be a useful accompaniment of ankle muscle action as in walking or jumping in order to achieve maximum lifting force. But both the flexors of the toes and the extensors of the ankle lie at the back of the leg, and their synergic contraction could not explain the recorded co-contraction of the antagonistic groups during flexion and extension at the ankle. Similarly, a maximum upward pressure of the foot may, and probably does, utilize extension of the toes, but the digital extensors of the foot lie at the front of the leg and lateral to the anterior tibialis and should not affect a lever resting on the tibialis proper. Even if they did, the synergic action would be recorded as a thickening of the tibialis, not as a contraction of its antagonistic muscle. In short, the recorded co-contraction of antagonists in the voluntary effort to flex and extend the mechanically immobilized foot is difficult to account for except as a true co-contraction of antagonistic muscles.

In the biceps-triceps and quadriceps-ham-string groups, both co-contraction and co-relaxation of antagonistics occurred. Two of our subjects showed co-contraction of antagonists of the biceps-triceps group in both flexion and extension of the mechanically immobilized forearm from previous relaxation. One subject showed co-contraction in the effort to extend and co-relaxation in the effort to flex the forearm. All our subjects showed coördinate relaxation of the ham-string muscles in the effort to extend the immobilized relaxed leg (Fig. 3c) and co-contraction of the quadriceps in the effort to flex it.

In view of the reflex relaxation of the ham-string muscles in the knee-jerk, and the persistence of the pattern of antagonistic action in whatever direction it occurs, one

conjectures that both co-contraction and co-relaxation depend on mechanisms at the lower levels of the nervous system, which may be set off by the voluntary effort to extend or flex a relaxed limb.

All this evidence for co-contraction and co-relaxation implies a more or less widespread wave of motor excitation in the voluntary flexion and extension of a mobile member. Just how widespread it is we do not know. It seems to correspond in the motor field to that phase of the reading process in which the first effect of words which are seen in peripheral vision before their direct visual fixation is neither meaningless nor negligible. It seems probable from experimental evidence that prefixational vision facilitates the process of apprehension in direct proportion to the proximity of a word to the area of clear vision; and that in the interplay of excitatory and inhibitory processes the preliminary prefixational vision produces a general excitation of related residua, which subsequent clear vision renders specific and definite. That seems to mean that the preliminary neural action corresponding to obscure prefixational vision is the general arousal of related systems, and that the subsequent clear vision operates to inhibit irrelevant, with consequent reinforcement of the relevant, factors.[15]

Our knowledge concerning the several factors in what may be an analogous complication in reflex, habitual, and voluntary behavior is altogether inadequate. Especially obscure is the voluntary regulation of behavior, both as to the nature and elaboration of the sensory controls, and as to the incidence and interaction of motor impulses, but it seems reasonable to suppose, if a complicated reaction is to be effected evenly and continuously, that there must be no real hiatus between the preliminary adjustments and their subsequent correction. Whatever the exact mechanism may prove to be, a widespread muscular innervation, including the co-contraction of antagonists, would seem to be an advantageous first step of a progressive adaptation. It would constitute a kind of protopathic adjustment or primitive

[15] Dodge, R., 'An Experimental Study of Visual Fixation,' PSYCHOL. MONOG., No. 35, pp. 51 *ff*.

reaction from which the epicritic or finer adjustments could be progressively elaborated.

4. The Development of Reciprocal Relaxation

While co-contraction of antagonistic muscles commonly appears in our records at the onset of voluntary extension or flexion of a relaxed limb, coördinate relaxation regularly appears when the antagonistic muscle happens to be caught in a condition of contraction at the moment of new innervation of the prime mover. This situation is represented in our records under two main conditions as follows: (1) In voluntary oscillation after the co-contraction of both antagonistics, the relaxation of either muscle group is commonly incomplete when any subsequent phase of rapid oscillation begins. Under such circumstances each new phase of overt movement is produced by the preponderant contraction of one or the other of the already partially contracted muscles, acting as a prime mover, while the contracted antagonist regularly suffers a partial relaxation. This sequence was more or less clearly perceptible by touch in the experiment with which this report began. Our records show it to be a regular phenomenon of rapid oscillation of all the mobile members which we investigated. If the oscillation is sufficiently slow, relaxation of both antagonistics may be completed before each new contraction begins. Under these circumstances each contraction episode starts with co-contraction, as though it were the first. But even during slow oscillation, if the co-contraction is voluntarily prolonged, each new phase may start just as a rapid oscillation with a residually contracted antagonistic which relaxes as a new phase begins. (2) Quite apart from all question of oscillation, the contraction of either one of the paired muscles may begin while its antagonistic is in a state of protracted contraction, as happened, for example, while maintaining a constant pressure of the limb against a stop preliminary to movement in the opposite direction. Whatever the origin of its contraction, a contracted antagonistic muscle regularly relaxed at each new voluntary contraction of a prime mover.

In the development of the muscular pattern of rhythmic rapid oscillation, reciprocal relaxation commonly appeared at the end of the first half oscillation. The first few waves of oscillation usually appear more or less confused and irregular. Regular patterns of thickening and thinning appear in our records at about the third oscillation, and then last as long as rapid oscillation continues. This is illustrated in Fig. 4 (*a*), which is a record of finger oscillation beginning with extension from relaxation. The first three double waves of extension

Extensors

Fig. 4 a Rapid
Finger oscillation
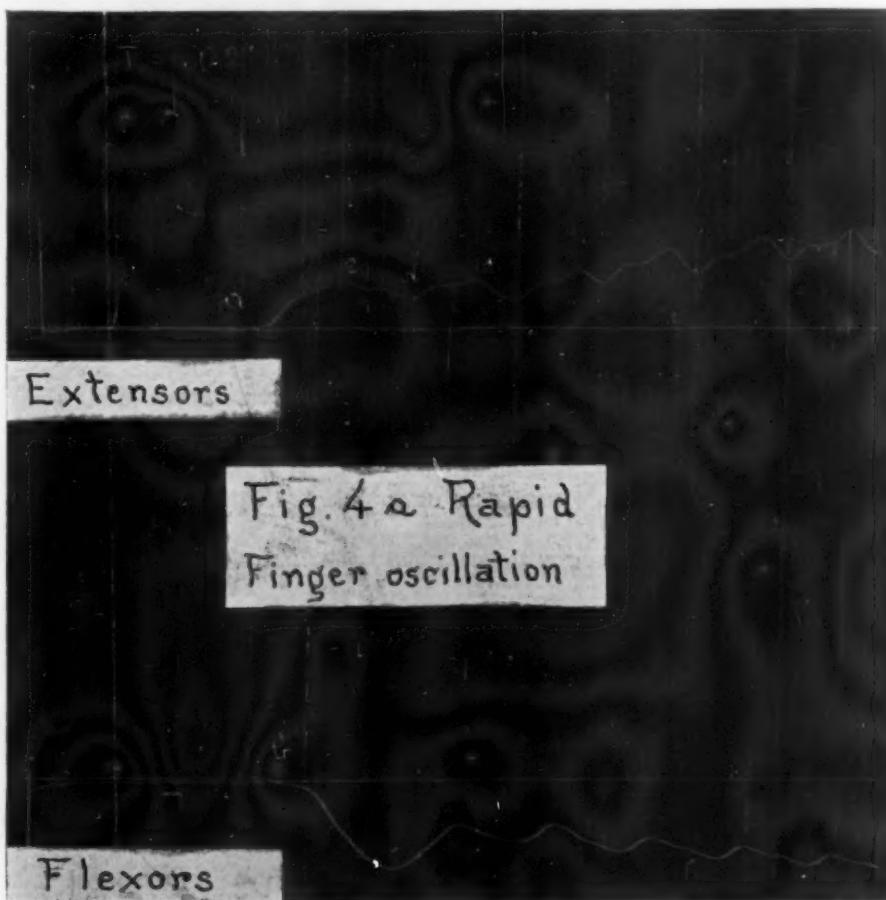
Flexors

Fig. 4*a*

thickening and thinning last respectively .264 sec., .170 sec., .136 sec. The tuning fork record has 50 D.V. per second. The record of muscle action begins at (*a*) according to rule with an episode of co-contraction of both the extensors and flexors, with emphasis on the extensors. A second episode of co-contraction begins when the emphasis shifts to the contraction of the flexors. Both initial co-contraction waves are dicrotic. The slope and extent of the excursions vary with the direction of emphasis. The peak of the extensor curve is reached about .04 sec. after the main wave of flexor contraction begins. The extensor record then passes into a dicrotic descending curve (*c*), which lasts until the second wave of extensor thickening begins at (*d*). This second wave lasts about .17 sec. It shows a dicrotic rise for about .10 sec. followed by a sharp descent becoming more gradual at the bottom. The antagonistic flexor is already contracted when the second extensor thickening wave begins and registers a coincident or slightly antecedent thinning, which passes into a thickening curve slightly ahead of the thinning of the extensor.

From this point on, the reciprocal contraction and relaxation follows a fairly regular pattern. Both extensor and flexor thickening curves are dicrotic and relatively long compared to the thinning curves; both thin during the period of thickening of the antagonistic main movers, but at different moments of that thickening. The thinning of the extensor begins during the first part of the flexor thickening curve; the thinning of the flexor during the last half of the dicrotic wave of extensor thickening. This gives a slightly arrhythmic appearance to the curves. The phenomenon seems to deserve records giving more exact time relations by techniques which are better adapted to that purpose.

Slowly alternating maximum extension and flexion, as shown in Fig. 4*b*, gives records of very different appearance, but with much the same fundamental characteristics as rapid oscillation. Starting from relaxation the curve of protracted extension, like the first curve in rapid oscillation, proceeds in waves. These waves are apparently simultaneous in both

antagonistics. They can hardly be referred to elastic oscillations of the two recording systems, since the latter are not identical either in weight or in pressure against the respective muscles. Furthermore, the waves continue into the subse-
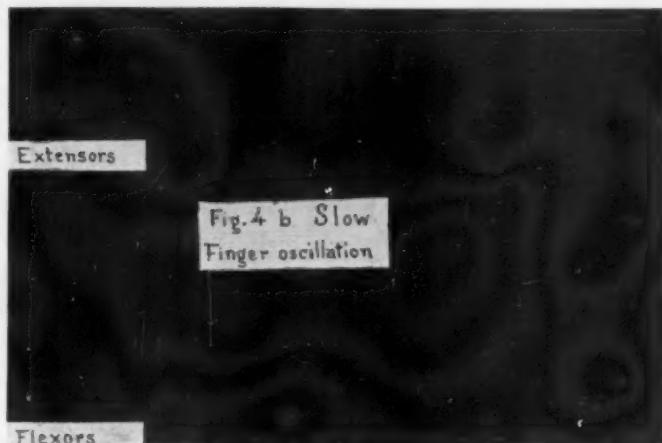
FIG. 4*b*

quent tetanic contraction, when mechanically conditioned elastic oscillations must long since have died out. They are too short for pulse waves and lack the characteristics of known proprioceptive reflexes. Their frequency, simultaneity, and persistence suggest that they are related in some way to the neural innervation waves, but this hypothesis needs careful scrutiny. If it is true, it would prove a common origin of the co-contraction impulses, beginning at the same moment and proceeding in waves which at first at least are practically simultaneous. Moreover, if such a spread of impulses to antagonistic muscles is granted, the contraction-relaxation relationship may be profoundly affected when innervation waves of different origin impinge on a part of the neuro-muscular system which is already stimulated in response to innervation waves of a different frequency. Something like this seems in fact to be indicated in our records of both slow and rapid oscillation subsequent to the initial co-contraction. The primary co-thickening of the antagonistic flexor, which

starts while the muscle is still uncontracted, is exactly co-incident with the onset of the contraction of the extensor. Relaxation of the antagonistic in subsequent phases is, however, seldom strictly simultaneous with the contraction of the antagonistic. In Fig. 4b, it is noticeably delayed about .025 sec. in the case of extensor relaxation and was delayed .06 sec. in the case of relaxation of the flexor, which is not shown in the cut. Other records show that the antagonistic relaxation may either precede or follow the thickening of the prime mover. These facts make a strong argument against any mechanical origin of these waves. It is however exactly what one might expect if the inhibition of the antagonist is a product of converging impulses that develop refractory phase. Even the irregularity in time of origin corresponds to this theory, but the whole matter is too important to rest on the available records. For the present we should limit our conclusions to the suggestive phenomenon that co-contraction of antagonistics is common when both muscles are previously relaxed, while relaxation is the rule when either one of the antagonistic pair is in sustained or residual contraction at the moment of dominant contraction of the other. This schema seems also to correspond to many of the phenomena of attention changes. But a further examination of this correspondence is outside our present task.

5. The Hypothetical Dual Origin of Muscle Contraction

Even so crude an indicator as palpation may discover a gradual increase in muscle thickening during oscillation of the finger, lasting for an appreciable time after oscillation ceases. Our records confirm this gradual thickening in both antagonistic muscles during rapid oscillation of a mobile member and show that between rapid oscillations of the finger neither extensors nor flexors completely relax. If one compares a curve of this partial relaxation between oscillations with the curve at the beginning of complete relaxation after oscillation ceases, as in Fig. 3 (a), it appears that the former is homologous with the latter. This means that each phase

of reciprocal relaxation has proceeded only part way toward complete relaxation when it passes into a contraction phase and that each new contraction phase of rapid oscillation is superimposed on a partial residual contraction of the relevant muscle. The sustained or more persistent contraction has the following peculiarities: (1) It increases at the beginning of rapid oscillation with each muscle contraction; (2) it gradually decreases at the end of oscillation or when the oscillation becomes sufficiently slow; (3) in no case is it apparently discontinuous from the rapid period of relaxation. Complete relaxation, consequently, discloses not separated episodes but variations in acceleration combined in one complex curve.

As far as we can interpret our records, they throw no light on the origin of the slowly increasing contraction or the variations in relaxation. They give no hint whether some new group of muscle fibres is brought into action at the beginning of oscillation with each successive wave, or whether many fibres participate in a gradually increasing tonic contraction. It is not impossible that we have come upon an illustration in voluntary behavior of the physiological theory that muscle contraction is a dual process in which a slow persistent factor called tonus arises in consequence of autonomic innervation and combines with a more rapid and less persistent factor of cerebrospinal origin to make up the total contraction.

BEARING OF OUR DATA ON THE LUCAS-FORBES HYPOTHESIS OF RECIPROCAL INHIBITION THROUGH REFRACTORY PHASE

The neural mechanism which provides for the alternation of flexor and extensor contraction and the relaxation of antagonistic muscles in voluntary action is still unknown, though there are various more or less plausible hypotheses. Psychologically it is quite inadmissible to presuppose separate acts of will or intent for each phase of the oscillation. There is introspective evidence that oscillation may be started or discontinued by voluntary intent, but it is probable that its

successive phases are controlled by some relatively automatic arrangement.

The evidence of a regular spread of innervation impulses to both antagonists seems quite clear. It shows itself in our records, in the initial simultaneous co-contraction of antagonists from a condition of relaxation and in the apparent coincidence of secondary waves of contraction in both. The same spread of impulses may, and probably does, occur when one of the pairs of antagonistic muscles is already contracted and the other is innervated anew. In this case, however, there usually develops a partial relaxation instead of a co-contraction of the antagonists. The one important difference in the two circumstances seems to be the fact of residual contraction.

The evidence for the existence of a kind of inhibition which is determined by refractory phase has been recently reviewed by Dodge.[16] Assuming here the possibility both of a refractory phase inhibition and of a spread of the motor impulses to both antagonists whenever either is innervated, there will be theoretical consequences which must be checked against the experimental facts. These consequences may be outlined as follows:

(1) An initial motor impulse to either flexion or extension, spreading to both antagonistic neuro-muscular systems when the relevant muscles are in a relatively relaxed condition, should result in an initial co-contraction with dominance of one or the other of the antagonistic muscles. Our records contain confirmatory evidence.

(2) If the original emphasis was on extension, a subsequent motor impulse, spreading to antagonistic motor systems, might find both in a state of residual excitation. Its effects may then be expected to vary with the relation between the force of the new excitation and the amount of residual excitation. A convergence on any part of the motor system of approximately equal impulses from two sources, especially if they were of unequal frequency, must do one of two things: either the phases will fall approximately together, so that one reinforces the other, or they will alternate and fall each

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[16] Dodge, R., 'Theories of Inhibition, II,' Psychol. Rev., 1926, 33, 167.

within the relative refractory phase of the other, producing inhibition. Both of these two consequences might appear in succession. This would lead us to expect records of relaxation of the original prime mover with occasioned beginnings of new co-contraction before relaxation sets in. These consequences are also found in our records. The key to reciprocal inhibition appears to lie in the spread of impulses, residual excitation, and a tendency of converging impulses from diverse sources to develop relative refractory phase.

(3) It would also seem to follow that whenever movement of a mobile member is voluntarily stopped suddenly the interaction of reinforcing and inhibiting impulses should cause oscillation of the limb in addition to the mechanical effects in a more or less elastic muscle system. This seems to be confirmed by the meticulous descriptions of Isserlin.[17]

(4) If both antagonistic systems are in a previous state of protracted balance, the theory would lead us to expect the frequent occurrence of beginning reactions in the wrong direction as in the negative reactions of Judd, McAllister, and Steele.[18] The refractory phase hypothesis of reciprocal relaxation of antagonistics in voluntary action cannot be said to be proven by our data. There are many gaps in the evidence and some apparently contradictory facts, like Fig. 3 (c). Perhaps the most important service of the discussion is to provide for the reformulation of the experimental question.

RESTATEMENT OF THE EXPERIMENTAL PROBLEM

On the basis of the data which are at present available the general problem with which we started—namely, what is the action of antagonistic muscles during voluntary extension and flexion, is capable of more precise formulation and subdivision into several partial problems. Some of these new formulations of the problem may be expressed as follows: (1) The precise time relations between the primary and secondary waves of voluntary muscle contraction and between

[17] Isserlin, M., 'Über den Ablauf einfacher willkürlicher Bewegungen,' Lpz., 1910.

[18] Monograph Supplements of the PSYCHOL. REV., 7, No. 1, pp. 141 ff.

voluntary muscle action and overt limb movement, pressure, and tremor; (2) the patterns of co-contraction and coördinate relaxation in both voluntary and reflex human behavior; (3) the influence of limb posture on the interplay of co-contraction and relaxation; (4) the range, limits, and conditions of voluntary dissociation and modification of the several phases of co-contraction and co-relaxation; (5) the muscle-motor patterns involved in suddenly stopping a moving limb in motion; and (6) in starting it in the same direction after it has been stopped. We believe that a study of these and the problems which may grow out of them is reasonably sure to increase our practical and theoretical knowledge of the mechanisms of human behavior. We feel that they might well be the nucleus of an extended co-operative program.

Outline

Our discussion may be outlined as follows:

1. An introductory experiment.
2. An effort to clarify relevant concepts.
3. A mechanical technique for studying muscular patterns of contraction and relaxation in voluntary action.
4. A list of records of voluntary oscillation variously conditioned.
5. The effect on the relevant muscles of slow passive oscillation.
6. Evidence for co-contraction of relaxed antagonistic muscles at the beginning of voluntary oscillation of a mobile member.
7. The conditions of the relaxation of antagonists and the origin of stable pattern of reciprocal contraction and relaxation in voluntary oscillation of a mobile member.
8. Some evidence in support of a dual system of muscle contraction in voluntary movement.
9. Extension of the hypothesis of refractory phase inhibition to account for the reciprocal relaxation of contracted antagonists in voluntary oscillation of a mobile member.
10. Restatement of our experimental problem.

A LAW OF COMPARATIVE JUDGMENT[1]

BY L. L. THURSTONE

The University of Chicago

The object of this paper is to describe a new psycho-physical law which may be called the *law of comparative judgment* and to show some of its special applications in the measurement of psychological values. The law of comparative judgment is implied in Weber's law and in Fechner's law. The law of comparative judgment is applicable not only to the comparison of physical stimulus intensities but also to qualitative comparative judgments such as those of excellence of specimens in an educational scale and it has been applied in the measurement of such psychological values as a series of opinions on disputed public issues. The latter application of the law will be illustrated in a forthcoming study. It should be possible also to verify it on comparative judgments which involve simultaneous and successive contrast.

The law has been derived in a previous article and the present study is mainly a description of some of its applications. Since several new concepts are involved in the formulation of the law it has been necessary to invent several terms to describe them, and these will be repeated here.

Let us suppose that we are confronted with a series of stimuli or specimens such as a series of gray values, cylindrical weights, handwriting specimens, children's drawings, or any other series of stimuli that are subject to comparison. The first requirement is of course a specification as to what it is that we are to judge or compare. It may be gray values, or weights, or excellence, or any other quantitative or qualitative attribute about which we can think 'more' or 'less' for each specimen. This attribute which may be assigned, as it were, in differing amounts to each specimen defines what we shall call the *psychological continuum* for that particular project in measurement.

As we inspect two or more specimens for the task of comparison there must be some kind of process in us by which we react differently to the several specimens, by which we identify the several degrees of excellence or weight or gray value in the specimens. You may suit your own predilections in calling this process psychical, neural, chemical, or electrical but it will be called here in a non-committal way *the discriminal process* because its ultimate nature does not concern the formulation of the law of comparative judgment. If then, one handwriting specimen *seems* to be more excellent than a second specimen, then the two discriminal processes of the observer are different, at least on this occasion.

The so-called 'just noticeable difference' is contingent on the fact that an observer is not consistent in his comparative judgments from one occasion to the next. He gives different comparative judgments on successive occasions about the same pair of stimuli. Hence we conclude that the discriminal process corresponding to a given stimulus is not fixed. It fluctuates. For any handwriting specimen, for example, there is one discriminal process that is experienced more often with that specimen than other processes which correspond to higher or lower degrees of excellence. This most common process is called here *the modal discriminal process for the given stimulus*.

The psychological continuum or scale is so constructed or defined that the frequencies of the respective discriminal processes for any given stimulus form a normal distribution on the psychological scale. This involves no assumption of a normal distribution or of anything else. The psychological scale is at best an artificial construct. If it has any physical reality we certainly have not the remotest idea what it may be like. We do not assume, therefore, that the distribution of discriminal processes is normal on the scale because that would imply that the scale is there already. We *define* the scale in terms of the frequencies of the discriminal processes for any stimulus. This artificial construct, the psychological scale, is so spaced off that the frequencies of the discriminal processes for any given stimulus form a normal distribution

on the scale. The separation on the scale between the discriminal process for a given stimulus on any particular occasion and the modal discriminal process for that stimulus we shall call *the discriminal deviation* on that occasion. If on a particular occasion, the observer perceives more than the usual degree of excellence or weight in the specimen in question, the discriminal deviation is at that instant positive. In a similar manner the discriminal deviation at another moment will be negative.

The standard deviation of the distribution of discriminal processes on the scale for a particular specimen will be called its *discriminal dispersion*.

This is the central concept in the present analysis. An ambiguous stimulus which is observed at widely different degrees of excellence or weight or gray value on different occasions will have of course a large discriminal dispersion. Some other stimulus or specimen which is provocative of relatively slight fluctuations in discriminal processes will have, similarly, a small discriminal dispersion.

The scale difference between the discriminal processes of two specimens which are involved in the same judgment will be called *the discriminal difference* on that occasion. If the two stimuli be denoted A and B and if the discriminal processes corresponding to them be denoted a and b on any one occasion, then the discriminal difference will be the scale distance $(a - b)$ which varies of course on different occasions. If, in one of the comparative judgments, A seems to be better than B, then, on that occasion, the discriminal difference $(a - b)$ is positive. If, on another occasion, the stimulus B seems to be the better, then on that occasion the discriminal difference $(a - b)$ is negative.

Finally, the scale distance between the modal discriminal processes for any two specimens is the separation which is assigned to the two specimens on the psychological scale. The two specimens are so allocated on the scale that their separation is equal to the separation between their respective modal discriminal processes.

We can now state the law of comparative judgment as follows:

$$S_1 - S_2 = x_{12} \cdot \sqrt{\sigma_1^2 + \sigma_2^2 - 2r\sigma_1\sigma_2,} \qquad (1)$$

in which

S_1 and S_2 are the psychological scale values of the two compared stimuli.

x_{12} = the sigma value corresponding to the proportion of judgments $p_{1>2}$. When $p_{1>2}$ is greater than .50 the numerical value of x_{12} is positive. When $p_{1>2}$ is less than .50 the numerical value of x_{12} is negative.

σ_1 = discriminal dispersion of stimulus R_1.

σ_2 = discriminal dispersion of stimulus R_2.

r = correlation between the discriminal deviations of R_1 and R_2 in the same judgment.

This law of comparative judgment is basic for all experimental work on Weber's law, Fechner's law, and for all educational and psychological scales in which comparative judgments are involved. Its derivation will not be repeated here because it has been described in a previous article.[2] It applies fundamentally to the judgments of *a single observer* who compares a series of stimuli by the method of paired comparison when no 'equal' judgments are allowed. It is a rational equation for the method of constant stimuli. It is assumed that the single observer compares each pair of stimuli a sufficient number of times so that a proportion, $p_{a>b}$, may be determined for each pair of stimuli.

For the practical application of the law of comparative judgment we shall consider five cases which differ in assumptions, approximations, and degree of simplification. The more assumptions we care to make, the simpler will be the observation equations. These five cases are as follows:

Case I.—The equation can be used in its complete form for paired comparison data obtained from a single subject when only two judgments are allowed for each observation such as 'heavier' or 'lighter,' 'better' or 'worse,' etc. There will be one observation equation for every observed proportion of judgments. It would be written, in its complete form, thus:

$$S_1 - S_2 - x_{12} \cdot \sqrt{\sigma_1^2 + \sigma_2^2 - 2r\sigma_1\sigma_2} = 0. \qquad (1)$$

[2] Thurstone, L. L., 'Psychophysical Analysis,' *Amer. J. Psychol.*, July, 1927.

According to this equation every pair of stimuli presents the possibility of a different correlation between the discriminal deviations. If this degree of freedom is allowed, the problem of psychological scaling would be insoluble because every observation equation would introduce a new unknown and the number of unknowns would then always be greater than the number of observation equations. In order to make the problem soluble, it is necessary to make at least one assumption, namely that the correlation between discriminal deviations is practically constant throughout the stimulus series and for the single observer. Then, if we have n stimuli or specimens in the scale, we shall have $\frac{1}{2} \cdot n(n-1)$ observation equations when each specimen is compared with every other specimen. Each specimen has a scale value, S_1, and a discriminal dispersion, σ_1, to be determined. There are therefore $2n$ unknowns. The scale value of one of the specimens is chosen as an origin and its discriminal dispersion as a unit of measurement, while r is an unknown which is assumed to be constant for the whole series. Hence, for a scale of n specimens there will be $(2n-1)$ unknowns. The smallest number of specimens for which the problem is soluble is five. For such a scale there will be nine unknowns, four scale values, four discriminal dispersions, and r. For a scale of five specimens there will be ten observation equations.

The statement of the law of comparative judgment in the form of equation 1 involves one theoretical assumption which is probably of minor importance. It assumes that all positive discriminal differences $(a - b)$ are judged $A > B$, and that all negative discriminal differences $(a - b)$ are judged $A < B$. This is probably not absolutely correct when the discriminal differences of either sign are very small. The assumption would not affect the experimentally observed proportion $p_{A>B}$ if the small positive discriminal differences occurred as often as the small negative ones. As a matter of fact, when $p_{A>B}$ is greater than .50 the small positive discriminal differences $(a - b)$ are slightly more frequent than the negative perceived differences $(a - b)$. It is probable that rather refined experimental procedures are necessary to

isolate this effect. The effect is ignored in our present analysis.

Case II.—The law of comparative judgment as described under Case I refers fundamentally to a series of judgments *of a single observer.* It does not constitute an assumption to say that the discriminal processes for a single observer give a normal frequency distribution on the psychological continuum. That is a part of the definition of the psychological scale. But it does constitute an assumption to take for granted that the various degrees of an attribute of a specimen perceived in it by *a group* of subjects is a normal distribution. For example, if a weight-cylinder is lifted by an observer several hundred times in comparison with other cylinders, it is possible to define or construct the psychological scale so that the distribution of the apparent weights of the cylinder for the single observer is normal. It is probably safe to assume that the distribution of apparent weights for *a group* of subjects, each subject perceiving the weight only once, is also normal on the same scale. To transfer the reasoning in the same way from a single observer to a group of observers for specimens such as handwriting or English Composition is not so certain. For practical purposes it may be assumed that when *a group* of observers perceives a specimen of handwriting, the distribution of excellence that they read into the specimen is normal on the psychological continuum of perceived excellence. At least this is a safe assumption if the group is not split in some curious way with prejudices for or against particular elements of the specimen.

With the assumption just described, the law of comparative judgment, derived for the method of constant stimuli with two responses, can be extended to data collected from a group of judges in which each judge compares each stimulus with every other stimulus only once. The other assumptions of Case I apply also to Case II.

Case III.—Equation 1 is awkward to handle as an observation equation for a scale with a large number of specimens. In fact the arithmetical labor of constructing an educational or psychological scale with it is almost prohibitive. The

equation can be simplified if the correlation r can be assumed to be either zero or unity. It is a safe assumption that when the stimulus series is very homogeneous with no distracting attributes, the correlation between discriminal deviations is low and possibly even zero unless we encounter the effect of simultaneous or successive contrast. If we accept the correlation as zero, we are really assuming that the degree of excellence which an observer perceives in one of the specimens has no influence on the degree of excellence that he perceives in the comparison specimen. There are two effects that may be operative here and which are antagonistic to each other.

(1) If you look at two handwriting specimens in a mood slightly more generous and tolerant than ordinarily, you may perceive a degree of excellence in specimen A a little higher than its mean excellence. But at the same moment specimen B is also judged a little higher than its average or mean excellence for the same reason. To the extent that such a factor is at work the discriminal deviations will tend to vary together and the correlation r will be high and positive.

(2) The opposite effect is seen in *simultaneous contrast*. When the correlation between the discriminal deviations is negative the law of comparative judgment gives an exaggerated psychological difference $(S_1 - S_2)$ which we know as simultaneous or successive contrast. In this type of comparative judgment the discriminal deviations are negatively associated. It is probable that this effect tends to be a minimum when the specimens have other perceivable attributes, and that it is a maximum when other distracting stimulus differences are removed. If this statement should be experimentally verified, it would constitute an interesting generalization in perception.

If our last generalization is correct, it should be a safe assumption to write $r = 0$ for those scales in which the specimens are rather complex such as handwriting specimens and childrens drawings. If we look at two handwriting specimens and perceive one of them as unusually fine, it probably tends to depress somewhat the degree of excellence

we would ordinarily perceive in the comparison specimen, but this effect is slight compared with the simultaneous contrast perceived in lifted weights and in gray values. Furthermore, the simultaneous contrast is slight with small stimulus differences and it must be recalled that psychological scales are based on comparisons in the subliminal or barely supraliminal range.

The correlation between discriminal deviations is probably high when the two stimuli give simultaneous contrast and are quite far apart on the scale. When the range for the correlation is reduced to a scale distance comparable with the difference limen, the correlation probably is reduced nearly to zero. At any rate, in order to simplify equation 1 we shall assume that it is zero. This represents the comparative judgment in which the evaluation of one of the specimens has no influence on the evaluation of the other specimen in the paired judgment. The law then takes the following form.

$$S_1 - S_2 = x_{12} \cdot \sqrt{\sigma_1^2 + \sigma_2^2}. \qquad (2)$$

Case IV.—If we can make the additional assumption that the discriminal dispersions are not subject to gross variation, we can considerably simplify the equation so that it becomes linear and therefore much easier to handle. In equation (2) we let

$$\sigma_2 = \sigma_1 + d,$$

in which d is assumed to be at least smaller than σ_1 and preferably a fraction of σ_1 such as .1 to .5. Then equation (2) becomes

$$\begin{aligned}
S_1 - S_2 &= x_{12} \cdot \sqrt{\sigma_1^2 + \sigma_2^2} \\
&= x_{12} \cdot \sqrt{\sigma_1^2 + (\sigma_1 + d)^2} \\
&= x_{12} \cdot \sqrt{\sigma_1^2 + \sigma_1^2 + 2\sigma_1 d + d^2}.
\end{aligned}$$

If d is small, the term d^2 may be dropped. Hence

$$\begin{aligned}
S_1 - S_2 &= x_{12} \cdot \sqrt{2\sigma_1^2 + 2\sigma_1 d} \\
&= x_{12} \cdot \sqrt{2\sigma_1}(\sigma_1 + d)^{\frac{1}{2}}.
\end{aligned}$$

Expanding $(\sigma_1 + d)^{1/2}$ we have

$$(\sigma_1 + d)^{1/2} = \sigma_1^{1/2} + \tfrac{1}{2}\sigma_1^{-(1/2)}d - \tfrac{1}{4}\sigma_1^{-(3/2)}d^2$$

$$= \sqrt{\sigma_1} + \frac{d}{2\sqrt{\sigma_1}} - \frac{d^2}{4\sqrt{\sigma_1^3}}.$$

The third term may be dropped when d^2 is small.   Hence

$$(\sigma_1 + d)^{1/2} = \sqrt{\sigma_1} + \frac{d}{2\sqrt{\sigma_1}}.$$

Substituting,

$$S_1 - S_2 = x_{12} \cdot \sqrt{2\sigma_1}\left[\sqrt{\sigma_1} + \frac{d}{2\sqrt{\sigma_1}} \right]$$

$$= x_{12}\left[\sigma_1\sqrt{2} + \frac{d}{\sqrt{2}} \right].$$

But $d = \sigma_2 - \sigma_1$;

$$\therefore S_1 - S_2 = x_{12}\frac{\sigma_2}{\sqrt{2}} + x_{12}\frac{\sigma_1}{\sqrt{2}}$$

or

$$S_1 - S_2 = .707x_{12}\sigma_2 + .707x_{12}\sigma_1. \tag{3}$$

Equation (3) is linear and very easily handled.   If $\sigma_2 - \sigma_1$ is small compared with σ_1, equation (3) gives a close approximation to the true values of S and σ for each specimen.

If there are n stimuli in the scale there will be $(2n - 2)$ unknowns, namely a scale value S and a discriminal dispersion σ for each specimen.   The scale value for one of the specimens may be chosen as the origin or zero since the origin of the psychological scale is arbitrary.   The discriminal dispersion of the same specimen may be chosen as a unit of measurement for the scale.   With n specimens in the series there will be $\tfrac{1}{2}n(n - 1)$ observation equations.   The minimum number of specimens for which the scaling problem can be solved is then four, at which number we have six observation equations and six unknowns.

Case V.—The simplest case involves the assumption that all the discriminal dispersions are equal.   This may be legitimate for rough measurement such as Thorndike's hand-

writing scale or the Hillegas scale of English Composition. Equation (2) then becomes

$$S_1 - S_2 = x_{12} \cdot \sqrt{2\sigma^2}$$
$$= x_{12}\sigma \cdot \sqrt{2}.$$

But since the assumed constant discriminal dispersion is the unit of measurement we have

$$S_1 - S_2 = 1.4142x_{12}. \tag{4}$$

This is a simple observation equation which may be used for rather coarse scaling. It measures the scale distance between two specimens as directly proportional to the sigma value of the observed proportion of judgments $p_{1>2}$. This is the equation that is basic for Thorndike's procedure in scaling handwriting and children's drawings although he has not shown the theory underlying his scaling procedure. His unit of measurement was the standard deviation of the discriminal differences which is $.707\sigma$ when the discriminal dispersions are constant. In future scaling problems equation (3) will probably be found to be the most useful.

Weighting the Observation Equations

The observation equations obtained under any of the five cases are not of the same reliability and hence they should not all be equally weighted. Two observed proportions of judgments such as $p_{1>2} = .99$ and $p_{1>3} = .55$ are not equally reliable. The proportion of judgments $p_{1>2}$ is one of the observations that determine the scale separation between S_1 and S_2. It measures the scale distance $(S_1 - S_2)$ in terms of the standard deviation, σ_{1-2}, of the distribution of discriminal differences for the two stimuli R_1 and R_2. This distribution is necessarily normal by the definition of the psychological scale.

The standard error of a proportion of a normal frequency distribution is

$$\sigma_p = \frac{\sigma}{Z} \cdot \sqrt{\frac{pq}{N}}.^3$$

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[3] See Kelley, T. L., 'Statistical Method,' p. 90, equation 43.

in which σ is the standard deviation of the distribution, Z is the ordinate corresponding to p, and $q = 1 - p$ while N is the number of cases on which the proportion is ascertained. The term σ in the present case is the standard deviation σ_{1-2} of the distribution of discriminal differences.   Hence the standard error of $p_{1>2}$ is

$$\sigma_{p1>2} = \frac{\sigma_{1-2}}{Z} \cdot \sqrt{\frac{pq}{N}}. \qquad (5)$$

But since, by equation (2)

$$\sigma_{1-2} = \sqrt{\sigma_1^2 + \sigma_2^2} \qquad (6)$$

and since this may be written approximately, by equation (3), as

$$\sigma_{1-2} = .707(\sigma_1 + \sigma_2) \qquad (7)$$

we have

$$\sigma_{p1>2} = \frac{.707(\sigma_1 + \sigma_2)}{Z} \cdot \sqrt{\frac{pq}{N}}. \qquad (8)$$

The weight, w_{1-2}, that should be assigned to observation equation (2) is the reciprocal of the square of its standard error.   Hence

$$w_{1-2} = \frac{1}{\sigma_{p1>2}^2} = \frac{Z^2 N}{.5(\sigma_1 + \sigma_2)^2 p \cdot q}. \qquad (9)$$

It will not repay the trouble to attempt to carry the factor $(\sigma_1 + \sigma_2)^2$ in the formula because this factor contains two of the unknowns, and because it destroys the linearity of the observation equation (3), while the only advantage gained would be a refinement in the weighting of the observation equations.   Since only the weighting is here at stake, it may be approximated by eliminating this factor.   The factor .5 is a constant.   It has no effect, and the weighting then becomes

$$w_{1-2} = \frac{Z^2 N}{pq}. \qquad (10)$$

By arranging the experiments in such a way that all the observed proportions are based on the same number of judgments the factor N becomes a constant and therefore has

no effect on the weighting. Hence

$$w_{1-2} = \frac{Z^2}{pq}.$$ (11)

This weighting factor is entirely determined by the proportion, $p_{1>2}$ of judgments '1 is better than 2' and it can therefore be readily ascertained by the Kelley-Wood tables. The weighted form of observation equation (3) therefore becomes

$$wS_1 - wS_2 - .707wx_{12}\sigma_2 - .707wx_{12}\sigma_1 = 0.$$ (12)

This equation is linear and can therefore be easily handled. The coefficient $.707wx_{12}$ is entirely determined by the observed value of p for each equation and therefore a facilitating table can be prepared to reduce the labor of setting up the normal equations. The same weighting would be used for any of the observation equations in the five cases since the weight is solely a function of p when the factor σ_{1-2} is ignored for the weighting formula.

Summary

A law of comparative judgment has been formulated which is expressed in its complete form as equation (1). This law defines the psychological scale or continuum. It allocates the compared stimuli on the continuum. It expresses the experimentally observed proportion, $p_{1>2}$ of judgments '1 is stronger (better, lighter, more excellent) than 2' as a function of the scale values of the stimuli, their respective discriminal dispersions, and the correlation between the paired discriminal deviations.

The formulation of the law of comparative judgment involves the use of a new psychophysical concept, namely, the *discriminal dispersion*. Closely related to this concept are those of the *discriminal process*, the *modal discriminal process*, the *discriminal deviation*, the *discriminal difference*. All of these psychophysical concepts concern the ambiguity or qualitative variation with which one stimulus is perceived by the same observer on different occasions.

The psychological scale has been defined as the particular linear spacing of the confused stimuli which yields a normal

distribution of the discriminal processes for any one of the stimuli. The validity of this definition of the psychological continuum can be experimentally and objectively tested. If the stimuli are so spaced out on the scale that the distribution of discriminal processes for one of the stimuli is normal, then these scale allocations should remain the same when they are defined by the distribution of discriminal processes of any other stimulus within the confusing range. It is physically impossible for this condition to obtain for several psychological scales defined by different types of distribution of the discriminal processes. Consistency can be found only for one form of distribution of discriminal processes as a basis for defining the scale. If, for example, the scale is defined on the basis of a rectangular distribution of the discriminal processes, it is easily shown by experimental data that there will be gross discrepancies between experimental and theoretical proportions, $p_{1>2}$. The residuals should be investigated to ascertain whether they are a minimum when the normal or Gaussian distribution of discriminal processes is used as a basis for defining the psychological scale. Triangular and other forms of distribution might be tried. Such an experimental demonstration would constitute perhaps the most fundamental discovery that has been made in the field of psychological measurement. Lacking such proof and since the Gaussian distribution of discriminal processes yields scale values that agree very closely with the experimental data, I have defined the psychological continuum that is implied in Weber's Law, in Fechner's Law, and in educational quality scales as that particular linear spacing of the stimuli which gives a Gaussian distribution of discriminal processes.

The law of comparative judgment has been considered in this paper under five cases which involve different assumptions and degrees of simplification for practical use. These may be summarized as follows.

Case I.—The law is stated in complete form by equation (1). It is a rational equation for the method of paired comparison. It is applicable to all problems involving the method of constant stimuli for the measurement of both

quantitative and qualitative stimulus differences. It concerns the repeated judgments of a single observer.

Case II.—The same equation (1) is here used for *a group* of observers, each observer making only one judgment for each pair of stimuli, or one serial ranking of all the stimuli. It assumes that the distribution of the perceived relative values of each stimulus is normal for the group of observers.

Case III.—The assumptions of Cases I. and II. are involved here also and in addition it is assumed that the correlation between the discriminal deviations of the same judgment are uncorrelated. This leads to the simpler form of the law in equation (2).

Case IV.—Besides the preceding assumptions the still simpler form of the law in equation (3) assumes that the discriminal deviations are not grossly different so that in general one may write

$$\sigma_2 - \sigma_1 < \sigma_1$$

and that preferably

$$\sigma_2 - \sigma_1 = d$$

in which d is a small fraction of σ_1.

Case V.—This is the simplest formulation of the law and it involves, in addition to previous assumptions, the assumption that all the discriminal dispersions are equal. This assumption should not be made without experimental test. Case V. is identical with Thorndike's method of constructing quality scales for handwriting and for children's drawings. His unit of measurement is the standard deviation of the distribution of discriminal differences when the discriminal dispersions are assumed to be equal.

Since the standard error of the observed proportion of judgments, $p_{1>2}$, is not uniform, it is advisable to weight each of the observation equations by a factor shown in equation (11) which is applicable to the observation equations in any of the five cases considered. Its application to equation (3) leads to the weighted observation equation (12).

ROBERT WHYTT: A CONTRIBUTION TO THE HISTORY OF PHYSIOLOGICAL PSYCHOLOGY [1]

BY LEONARD CARMICHAEL

Princeton University

To the professed psychologist by no means exclusively belongs the credit for developing a scientific understanding of the nature of mental life; nor does the history of philosophy include all of the chapters which are significant for those who would learn of the discovery of the rugged facts which form the foundation stones of modern psychology. This paper presents an outline of the life and works of Robert Whytt. This physician and physiologist claimed to be neither psychologist nor philosopher, and yet he did add to the basic knowledge of the response mechanism. For this contribution he deserves, if for no other reason, consideration in a history of physiological psychology.

I. BIOGRAPHICAL INTRODUCTION

Robert Whytt was a member of an old and honorable family in Scotland.[2] His father died before Robert was born.

[1] This paper is the second of a series of contributions to the history of physiological psychology. The previous paper on 'Sir Charles Bell' appeared in the PSYCHOL. REV., 1926, **33**, pp. 188–217.

[2] Biographical facts in regard to Robert Whytt (pronounced "White") are not readily available. The best published source of information, and the one upon which most other biographical notices of Whytt are based, is a Makdougall Brisbane Prize Essay by William Seller, entitled, 'Life and Writings of Robert Whytt, M.D.' *Trans. Roy. Soc. Edinburgh*, 1864, **23**, pp. 99–131. A condensed life by George Stronach appears in the Dictionary of National Biography 1900, **61**, pp. 174 *f*. Dr. Ruhräh has twice written on the life and work of Whytt, his first paper being undertaken at the advice of Dr. Osler. (*Cf.* Ruhräh, J., 'Robert Whytt, M.D., Professor of Medicine, University of Edinburgh, 1747–1766, *Medical Library and Historical Journal*, 1904, **2**, pp. 153–160, and *idem*, 'Pediatrics of the Past,' 1925, pp. 401–422.) In both of these articles there is given a full-page reproduction of a photograph of the portrait of Whytt, 'after Bellicci' which hangs in the halls of the Royal College of Physicians in Edinburgh. So far as the present writer has been able to determine these are the only printed reproductions of Whytt's features, and these are not listed in the indices dealing with such matters. Other references to the life of Whytt are found in the 'Dictionnaire Historique de la

Possibly for this reason everything seems to have been done for the education and true advancement of the posthumous child. Early he distinguished himself for his learning. When but sixteen years of age he was made by the University of St. Andrews a Master of Arts. After receiving this degree he proceeded at once to the study of medicine. First he attended the sessions in the still new, but already justly famous, medical school of the University of Edinburgh. Here he was fortunate enough to select for his particular preceptor that famous anatomist, the elder Dr. Monro.[3]

Having succeeded to the family estates the young medical student, with ample means at his disposal, left Edinburgh at the age of twenty and proceeded to London where he studied for a time. After this he continued his work at Paris, Leyden and the other medical centers of the continent. Whytt finally completed his formal preparation for a medical career by taking the degree of Doctor of Medicine at Rheims in 1736.[4] In the following year the same degree was conferred upon him by St. Andrews. In 1738 he was elected fellow of the Royal College of Physicians of Edinburgh.

Whytt twice married. His first wife was a sister of General Robertson, Governor of New York. His second wife, Miss Balfour, was a sister of a Professor in the University of Edinburgh. He was the father of sixteen children.

From 1743 until the very year of his death he continued

Medecine Ancienne et Moderne' par J. E. Dizeimeris, 1839, 4, pp. 404 *f*.; 'Biographie Universelle' (Renauldin), 1827, 50; 'An Introduction to the History of Medicine,' 3d ed., 1921, pp. 332 *f*. (by Garrison, F. H.); and short notices in various biographical dictionaries. After the present paper was in proof a further article on Whytt came to my notice. *Cf.* Comrie, J. D. 'An Eighteenth Century Neurologist,' *Edinburgh Med. J.*, 1925, New Series 32, pp. 755–761. Here the Edinburgh portrait is again reproduced, and a picture of Whytt's tombstone in Greyfriars Churchyard is inserted.

[3] Whytt received instruction from many of the famous medical teachers of his day. All of these are mentioned by Seller, *op. cit.*, pp. 100 *f*. and presumably from this list Garrison notes, as particularly worthy of mention (*op. cit.*, p. 332), Cheselden, Winslow, Boerhaave, and Albinus.

[4] An explanation of why Whytt who, though wealthy, was after all a Scotchman, did not take his degree at Leyden was suggested to Dr. Ruhräh by Dr. Osler who gave him a quotation from that 'gossipy parson physician' Dr. John Ward, as follows: "Doctor's degrees at Leyden cost sixteen pounds besides the feasting of the faculty; at Angers not above nine pounds and feasting not necessary neither." (*Cf.* Ruhräh, J., 'Robert Whytt,' etc., *op. cit.*, p. 154.)

to publish important papers on various subjects in medicine and physiology.[5]

In 1747 he was made Professor of the Theory of Medicine in the University of Edinburgh. His lectures were given in Latin. They have been characterized as rhetorically brilliant and scientifically valuable. In 1752 he was made a Fellow of the Royal Society of London. In 1761 he was made first physician to the King in Scotland, an office which seems to have been created for his honor. Two years after receiving this distinction he was elected to the presidency of the Royal College of Physicians in Edinburgh, an office which he continued to hold at the time of his death in 1766.

[5] Bibliographies of Whytt's works are to be found in several places, for example, in Watt's *Bibliotheca Britannica*. Many of his books and papers appeared in several rather thoroughly revised editions and a number have been translated into German and French. Almost all of his writings, however, in their definitive form were collected and published by his son with the assistance of Sir John Pringle, under the title of 'The Works of Robert Whytt, M.D.,' 1768. The works included in this compilation are given below with the pagination there used. The references made in this paper are to the reprints in the 'Works,' unless noted to the contrary, and the pages cited are to that edition. A German translation of the complete works was published in Leipzig in 1771. The 'Works' include the following items:

(1) 'An Essay on the Vital and Other Involuntary Motions of Animals,' 1st ed. 1751, 2d ed. 1763, pp. 1–208.

(2) 'Physiological Essays,' containing, (1) An Inquiry into the Causes which Promote the Circulation of the Fluids in the very Small Vessels of Animals. (2) Observations on the Sensibility and Irritability of the Parts of Men and Other Animals, 1st ed. 1755, 2d ed. 1759, 3d ed. 1766, pp. 209–306.

(3) 'An Account of Some Experiments Made with Opium,' etc. (Date of first publication in question.) Pp. 307–327.

(4) 'An Essay on the Virtues of Lime-water and Soap in the Cure of the Stone,' 1st ed. 1752, 2d ed. 1754, 3d ed. 1761, pp. 328–429. (Appended to this is an account of the treatment of three cases of stone, one with a letter from Horace Walpole.) Pp. 328–465.

(5) 'An Essay on the Various Strengths of Different Lime-waters.' (Date of first publication in question.) Pp. 466–476.

(6) Extracts from letters of 1757, pp. 477–486.

(7) 'Observations on the Nature, Causes, and Cure of those Disorders which are commonly called Nervous, Hypochondriac or Histeric, 1st ed. 1764, 2d ed. 1765, 3d ed. 1767, pp. 487–713.

(8) 'Cases on the Effect of Blisters,' 1757, pp. 715–722.

(9) 'Observations on the Dropsie in the Brain' (published for the first time in the Works), pp. 723–745.

(10) Appendix with an account of an Epidemic, etc., pp. 746–762.

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II. The Response Mechanism

While of undoubted importance the exact contribution of Robert Whytt to the knowledge of the reflex arc is not easy to delineate. In evaluating his work the advances which had been made before his time must not be underestimated nor must his contributions be given by accentuation, an importance which they do not deserve. After his death there still remained much to be done before the current conception of response could be established.

In a previous paper reference has been made by the present writer to the early history of the doctrine of reflex action. The name of Descartes [6] is quite properly always associated with the origin of this theory. It is interesting to notice, however, that Thomas Willis suggested as an analogy for physiological response not the reflection of light, but the rapid return of sound; he spoke of the response following a stimulus as an echo.[7] Immediately after the suggestive work of these two men there follows a whole series of writers who speculated upon the phenomena of reflex action.[8] Many of these writers were students of anatomy who made more or less fanciful efforts to guess the functions of the parts which they dissected. So far as their opinions upon response are concerned these early thinkers may be divided into two hostile groups. First, there were those who believed that the central nervous system, or, in the strict Cartesian sense, some particular part of the central apparatus, was necessary for reflex action. Second, there were writers who held that the observed reactions might be accounted for by the reflection of the 'nervous fluid' in the apparent anastomosis of peripheral

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[6] Carmichael, L., *op. cit.*, pp. 191 *ff.*

[7] *Cf.* Eckhard, C., 'Geschichte der Entwickelung der Lehre von den Reflexerscheinungen, III, Beiträge zur Geschichte der Experimental-physiologie des Nervensystems,' *Beiträge z. Anat. u. Physiol.*, von C. Eckhard, Giessen, 1881, 9, p. 36, and Hodge, C. F., 'A Sketch of the History of Reflex Action,' *Amer. J. Psychol.*, 1890, p. 156.

[8] Concerning this development Hodge has well said (*op. cit.*, p. 150), "It is a great descent from the clear views of Descartes to the obscure ideas of his contemporaries and even of those who follow him. But there are compensations, for we shall exchange in a measure the highways of philosophy for the by-paths of experiment."

nerves. These latter thinkers made much capital of the observed 'sympathies' of the body.[9]

Robert Whytt may be given the credit for solving this dilemma and for establishing by experimental means the fact that the central nervous system is the essential reflex center of the body.[10] The fundamental experiment which established this fact, however, was not devised by Whytt, but as he himself generously and candidly admits, by Stephen Hales. Concerning the origin of this basic empirical observation Whytt says, "The late Reverend and learned Dr. Hales informed me, that having many years since tied a ligature about the neck of a frog to prevent any effusion of blood, he cut off its head, and, thirty hours after, observed the blood circulating freely in the web of the foot . . . but that, on thrusting a needle down the spinal marrow, the animal was strongly convulsed, and immediately after became motionless." [11]

Stephen Hales (1677–1761), referred to in the above quotation was, besides an active parish minister, a man of original scientific mind. His contributions to plant physiology and to the nature of the circulation of the blood are well known. As Whytt's excerpt shows, however, Hales' first interest in the experiment under consideration was in the circulation of the blood and not in the nervous system. In fact it remained for Whytt to give a systematic setting to

[9] These early neurologists have been divided in regard to their views upon this subject by Eckhard, *op. cit.*, p. 38, and by Sherrington, C. S., in Schäfer's 'Text-Book of Physiology,' 1900, p. 786. The two lists are not quite the same. Eckhard includes more of the continental writers while Sherrington adds a few English names.

[10] Eckhard (*op. cit.*, p. 43) mentions Whytt's name in connection with this first experimental demonstration. Luciani says: "Whytt, 1750, was the first who demonstrated that the agency of a central organ is necessary for the transmission of excitations from afferent to efferent nerves. As soon as the gray matter of the cord is destroyed every reflex movement ceases. This same author showed that reflex action does not depend on the integrity of the cord as a whole, but that an isolated segment suffices for the reaction. . . ." ('Human Physiology,' 1917, p. 311.)

[11] Whytt, 'Physiological Essays,' p. 290. I am indebted to Dr. J. F. Fulton for calling to my notice the fact that Hales wrote, in his *Haemastatiks*, 1733, pp. 61f., concerning the behavior of muscle fibers observed in action in a live frog held in a frame under a microscope. In looking at the muscles of the belly of such an animal he notes that, " Stimulating the foot of a Frog, will sometimes make it contract these muscles."

Hales' isolated observation and to show its importance for neural physiology.[12]

Whytt repeated this experiment and explained the results in the following language:

Further, it ought to be observed, that when, after decollation, the spinal marrow of a frog is destroyed with red hot wire, no visible motion is produced in its limbs or body, by pricking, cutting or otherwise hurting them. . . . It seems also to deserve notice, that, after the destruction of the spinal marrow . . . there was no sympathy between the different muscles, or other parts of the body, as was observed when the spinal marrow was entire: from whence it seems to follow, that the nerves distributed to the several parts of the body have no communication but at their termination in the brain or spinal marrow; and that to this, perhaps alone, is owing the *consent* or sympathy observed between them.[13]

This has been termed the fundamental experiment of reflex physiology.[14] Its basic nature lies in the fact that by the technique here employed it was demonstrated for the first time that the central apparatus was essential to neuro-muscular response in the organism.

Before the time of Hales and Whytt it had been known that reflex sensitivity remained in decapitated animals. Thus Robert Boyle had already pointed out that decapitated vipers would continue to respond to stimulation for days after they had been operated upon.[15] This observation, however, did not show as did the later 'fundamental experiment' that the integrity of the central nervous system was essential to response. Moreover, even to the experiment of Hales, Whytt added the observation that a small segment of the cord was sufficient to allow reflex action. This Sherrington contends is Whytt's fundamental contribution to neurology.[16] The

[12] For facts in regard to Hales, 'the overzealous rector of Tedington,' *cf.* an article by Francis Darwin in the *National Dictionary of Biography*, 1890, 24, pp. 32–36. It is interesting to note in passing that Hales was probably the first one who ever suggested that the nervous impulse was electrical in nature.

[13] Whytt, 'Physiological Essays,' pp. 284 *f.*

[14] *Cf.* Sherrington, C. S., *op. cit.*, 786, and Eckhard, C., *op. cit.*, p. 43.

[15] *Cf.* Eckhard, C., *op. cit.*, p. 38.

[16] Sherrington says (*op. cit.*, p. 811): "Whytt was the first to show that the integrity of even a fraction of the length of the spinal cord suffices to enable reflex

experiment on which Whytt based this generalization was done on a frog.[17]

It must not be supposed, of course, that as a result of these experiments Whytt arrives at a modern theoretical explanation of reflex action. Concerning his method of work he tells us that "he has been careful not to indulge his fancy, in wantonly framing *hypotheses*, but has rather endeavored to proceed upon the surer foundations of experiment and observation."[18] In spite of this worthy resolution Whytt did present much theory, and some of his theorizing seems bizarre indeed to modern eyes. The work of Whytt is in a measure ironical; the facts that he discovered were later to destroy the theories that he held most dear. He taught that the Cartesian view of the body as a mere machine was most ridiculous.[19] As he, himself, says: "Every attempt hitherto made towards deducing the vital motions of animals from powers wholly material, has been unsatisfactory, and I believe that I may venture to say, will be forever in vain."[20] The facts, however, that he first clearly stated have done much to make possible a mechanistic physiology of the nervous system.

III. WHYTT'S THEORY OF RESPONSE

The fact that Whytt did not achieve a really scientific view of the problems that he envisaged is not surprising. Save for the speculations of a few radical Cartesians,[21] this was a time of grotesque animism in science. When Whytt began his work, response was regarded by competent au-

reactions to occur in response to skin stimuli, and therefore that reflex function does not depend on the integrity of the cord as a whole."

[17] Whytt, 'Essay on the Vital and other Involuntary Motions of Animals,' p. 203. For an admirable summary of Whytt's contributions to the physiology of response *cf.*, Fulton, J. F., 'Muscular Contraction and the Reflex Control of Movement', 1926, pp. 32*ff.*

[18] Whytt, 'Works,' p.v. of preface to 'Essay on Vital and other Involuntary Motions of Animals.'

[19] Whytt, *op. cit.*, p. 16, and *idem*, 'Physiological Essays,' p. 290.

[20] *Idem*, 'Essay on the Vital and other Involuntary Motions of Animals,' p. 145.

[21] *Cf.* Lange, F. A., 'The History of Materialism' (Translated by E. C. Thomas), 1925, 1, pp. 246 *ff.*

thorities as what must be termed a magic performance. For example, the doctrine of 'sympathy,' which included many vital responses, was held to be dependent upon purely verbal explanations. Instead of depending upon physiological response to stimulation it was seriously suggested that vital reactions depended upon such entities as 'situation,' 'connection,' 'vicinity,' 'similarity,' and 'like function.' Certain distinguished writers, too, insisted on the significance of mysterious vapours as the basis of response. Whytt definitely pointed out the unsatisfactory nature of these speculations.[22] He asserted that *sympathy* must depend upon nerves, and upon nerves that were connected in the brain or spinal marrow.[23] Even though for him these very nerves were not merely mechanical connectors, the advance over the older purely mystical view is obvious. The complication of the central nervous system has always provided an opportunity for the 'vitalistically minded' to protest that ordinary mechanistic concepts do not apply to its functions. This was true in the case of Whytt. For him the central apparatus, the essential nature of which he demonstrated, was not considered as a mere system of interconnecting fibers. On the contrary he held that it was the seat of some non-physical principle. Even in the cord he held that a sentient principle was essential to effect the connection between afferent and efferent pathways.[24]

In passing it is worthy of note that for Whytt apparently all peripheral nerves might serve interchangeably for afferent and efferent functions. In fact, the important truth of the specificity of the sensory and motor nerves was not made clear until the time of Sir Charles Bell.[25]

Whytt's conception of the rôle of muscles in response is interesting and in a measure modern. In regard to tonus, for example, he writes: "The natural contraction of the muscles is, in a great measure, to be ascribed to the influence

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[22] *Idem*, 'Observations on the Nature, Causes, and Cure of those Disorders which are commonly called Nervous,' pp. 504 *f.*

[23] *Ibid*, p. 510.

[24] Whytt, *loc. cit.*

[25] Carmichael, L., *op. cit.*, p. 192.

of the nerves, which is perpetually operating upon them, though in a very gentle manner. . . ." [26] He was also familiar with the fact that stretching a muscle stimulated it.[27] It is significant of the state of the knowledge of the response mechanism at the time Whytt was writing that he found it necessary to present experimental evidence that sudden muscular contraction in the living animal was due to the influence of the nerves and not of the blood vessels.[28] Whytt also noted a phenomenon of inhibition. Responses he found could not be elicited for a short period after decapitation.[29] This phenomenon is really one of 'shock' although the term was not introduced until 1850 by Marshall Hall.[30]

It is difficult to determine exactly what Whytt considered to be the connection between the nervous system that he saw in his dissections and the soul that he postulated in his theories. In the first place it must appear that Whytt was no slavish follower of the vitalism or even the semi-animism of Stahl, who held that involuntary movements were caused by the rational soul.[31]

There can be no doubt, nevertheless, that Whytt held what we should today call a vitalistic view of the nervous system; and yet he does qualify his beliefs as follows: "The immediate cause of muscular contraction, which, from what has been said, appears to be lodged in the brain and nerves, I choose to distinguish by the terms of *power* or *influence of*

[26] Whytt, 'An Essay on the Vital and other Involuntary Motions of Animals,' p. 8.

[27] *Ibid.*, pp. 9 *f.*

[28] *Ibid.*, pp. 6 *f.*

[29] *Idem*, 'Physiological Essays,' p. 291 and 'Observations on the Nature, Causes and Cure of those Disorders which are commonly called Nervous,' p. 501.

[30] *Cf.* Sherrington, C. S., *op. cit.*, p. 845.

[31] Sellers, W. (*op. cit.*, pp. 99 *f.*) has taken great pains to defend the subject of his essay against this accusation. Moreover, Whytt himself says that Stahl extended the supposed influence of the soul 'as a rational agent over the body a great deal too far' (an 'Essay on the Vital and other Involuntary Motions of Animals,' pp. 140 *f.*) Garrison also notes that Whytt was moved to discard the current hypothesis of Stahl that a rational soul is the cause of involuntary movements (*op. cit.*, 332). For a general outline of this development *cf.* McDougall, W., 'Body and Mind,' 1911, Chaps. V. and VIII. *Cf.* also, Gault, R. H., and Howard, D. T., 'Outline of General Psychology,' 1925, p. 439 and p. 448.

the nerves; and if, in compliance with custom, I shall at any time give it the name of *animal* or *vital spirits,* I desire it may be understood to be without any view of ascertaining its particular nature or manner of acting; it being sufficient for my purpose that the existence of such a power is granted in general, though its particular nature and properties be unknown." [32]

At a later point, however, he does say in regard to the nature of the soul, "If I were allowed a conjecture concerning a matter of which I know very little, I would say, that when a nerve is pricked below where it is cut or tied; although there can be no feeling or perception in the brain yet, if the soul be present everywhere in the body, as seems probable, there may be some kind of feeling or sensation excited in the nerve itself, which may be sufficient to produce a motion in the muscles to which it belongs. . . . As the soul seems to imagine, judge, reason and remember in the brain only; why might it not have, in the various parts of the body such feelings or powers as are necessary for carrying on their several functions?" [33]

In spite of the apparent assertion to the contrary Whytt held that the soul, while it might be present in any part of the animal was, nevertheless, indivisible. [34]

Reduced to its simplest terms, then, it seems that Whytt held that, in a typical reflex, the nervous fluid passed into the central apparatus and there was transferred by a 'sentient principle' or 'sentient being' into the appropriate channel of egress to the muscles or glands involved.

At first glance this view of Whytt's may seem about as pure animism as one can imagine; but as a matter of fact, as suggested above, it was really an advance toward a true mechanistic explanation of response. An example may make this more clear. According to the older view of Stahl, that Whytt combatted, if hot water fell upon the foot it was

[32] *Idem,* p. 6.

[33] *Idem,* 'Physiological Essays,' p. 287 (note).

[34] His metaphysical reasoning upon this point as well as upon the question of the extent of the soul is non-scientific in tone and is couched in the language of the Schoolmen.

withdrawn by a conscious willing on the part of the individual. According to Whytt's view on the contrary this action was quite automatic, but the mechanism of the response involved at one place a non-physical principle. A next step in scientific development could thus be taken easily. It was only necessary to show neural continuity with no non-physical intervention at the spinal or other level in order to have the first scientific view of simple response. It is generally held that this apparently obvious advance was not made until the time of Dr. Marshall Hall.[35] It has been suggested, however, that before Hall's publication, indeed, as early as 1826, Sir Charles Bell had declared in public lecture that what he termed the 'nervous circle' might be completed at the central apparatus without any non-physical intervention.[36] A search of Whytt's works, however, fails to disclose any adumbration of this view. He established the fact that the central nervous system was necessary in response, but concerning the scientific nature of the central apparatus, itself, he was unenlightened.

Whytt taught there were three kinds of response observable in the muscles of animals: (1) 'Natural,' that is, all muscles 'are constantly endeavoring to shorten or contract themselves'; (2) 'Voluntary,' that is, excited by the power of the will; and (3) 'Involuntary,' that is, dependent upon a stimulus directly applied to the muscle or to the nerve of that muscle.[37]

Now each of these classes of responses, in the light of the psychophysical assumptions considered above, must appear as dependent upon certain agencies which are not merely mechanical. In fact it is only possible to evaluate correctly Whytt's speculations upon physical or mental phenomena if these assumptions are clearly kept in view. It is significant of Whytt's general sanity of judgment however that he

[35] *Cf.* Hall, M., 'On the Reflex Functions of the Medulla Oblongata and the Medulla Spinalis,' 1833, pp. 638 *ff.* It seems probable that Hall was the first to publish upon this matter. *Cf.* also, Du Bois-Reymond, E., 'Gedächtnisrede auf Joh. Müller,' Abhandlungen der Königlichen Akademie der Wissenschaften zu Berlin, 1859, pp. 77 *f.*

[36] *Cf.* an unsigned article, 'Sir Charles Bell,' *Quart. Rev.*, 1843, p. 72.

[37] *Ibid.*, pp. 7–16.

remarks that the study of involuntary movement seems more important than that of voluntary movement. Later research has demonstrated the fact that it is true, indeed, that simple response must be understood before the complex and so-called 'voluntary action' may be understood in scientific terms. In regard to this matter he says, "The muscular fibers of animals are so framed, as to contract whenever a cause proper to excite their action is applied to them, or, in defect of this, always to remain at rest. This cause is either an effort of the will or a *stimulus* of some kind or another: to the former are owing the voluntary motions; and to the latter all such as we call vital and spontaneous. How, or in what manner the will acts upon the voluntary muscles, so as to bring them into contraction, is a question beyond the reach of our faculties; and, indeed, were it otherwise, the answer would be of no great importance, it being sufficient that experience convinces us that the will is really possessed of that power." [38]

IV. MISCELLANEOUS CONTRIBUTIONS

The reflex contraction of the pupil to light is known as Whytt's reflex.[39] Concerning this phenomenon Whytt points out that if the pupil were not capable of 'enlargement or diminution' the eye would have been adapted to contemplate objects 'in one particular degree of light.'[40] Moreover, he attributed to the changes in pupillary size the function of accommodation.[41] He says, "Further, as the rays of light coming from near objects, are much more divergent than those from remote ones, had the pupil been incapable of variation as to its extent, the eye would have been ill-fitted for seeing distinctly at different distances; since such objects alone are seen distinctly whose images are painted upon the

[38] *Ibid.*, p. 122.

[39] Sherrington, C. S., in Schäfer's 'Text-Book of Physiology,' p. 913.

[40] Whytt, 'An Essay on the Vital and Other Involuntary Motions of Animals,' pp. 59 f.

[41] Whytt was not ignorant of the fact that the contraction of the ciliary process changed the shape of the 'crystalline humour,' but he apparently thought of this as secondary to the changes in size of the pupil. *Cf. ibid.*, pp. 76 f.

middle and most sensible parts of the *retina*." [42] After a consideration of the comparative anatomy of the iris in lower forms, and the history of the teaching in regard to the iris since the time of the Greeks, he proceeds to examine the mechanism of the iris. He concludes that the iris does not change in shape because of local stimulation, but because of a central reflex which is initiated by the stimulation of the retina. He explains this phenomenon in the following words: [43] "Since the optic nerves and those of the *uvea* arise from different parts of the brain, and have no communication with each other in their course to the eye, it seems evident, that light affecting the *retina* cannot excite the *sphincter* of the pupil into contraction by any immediate mechanical change which it produces, either in the muscle itself, or in the nerves which actuate it; but the uneasy sensation occasioned in the *retina* by the admission of too much light into the eye, may so affect the sentient principle, which is present and ready to act wherever the nerves have their origin, as to excite it to contract the orbicular muscle of the *uvea* in order to lessen the pupil, and exclude the offending cause. While the eye remains in the same degree of light, and directed towards the same object, the pupil remains invariably of an equal size, as the same cause continues uniformly to excite the mind to determine the nervous influence in the same degree into its *sphincter* muscle: but no sooner does the light become fainter, than the sentient principle being less affected, ceases to contract this muscle and allows the curtain of the pupil to be opened by the natural action of its longitudinal fibers, by which means more light is admitted to the eye." [44]

[42] *Ibid.*, p. 58.

[43] It is interesting to notice that recent work has shown that the contraction of the iris is at least potentially open to direct stimulation and response independent of all nervous connection. *Cf.* Parker, G. H., 'The Elementary Nervous System,' 1919, pp. 50–53. As is shown below Whytt was not ignorant of the fact that 'independent response' was possible.

[44] Whytt, 'An Essay on the Vital and Other Involuntary Motions of Animals,' pp. 64 f. The author gives what may be termed a hedonistic explanation of the 'cause' of this action. "Such," he says, "is the original constitution of our frame that the mind or sentient principle is, in consequence of certain uneasy sensations, instantly determined to produce such motions or changes in the body as tend to remove or lessen them" (*loc. cit.*).

He gives the following example of the action of the pupil as he understands it: "Thus if one with his back to the windows of a room, brings a small printed book so near his eyes that he cannot without straining distinguish the letters; upon turning his face quickly to the light he will be able to read with little difficulty; because by the action of the stronger light on the *retina*, the pupil is immediately lessened. . . ." [45]

Whytt also speculated upon the reflex mechanism of the inner ear as follows: "As, without the motions of the pupil, the eye would have been ill contrived for vision in different degrees of light, and at different distances; so the ear would have been unfit for hearing distinctly a diversity of sounds, were not some of its parts capable of various degrees of tension. A musical cord of a determinate length and tension can only vibrate harmonically with one particular sound; if therefore there was no mechanism, by means of which the membranes of the *tympanum* and *fenestra ovalis* could be more or less stretched or relaxed, they could only be harmonically affected by one sound; which, therefore alone would be heard distinctly, and all others more or less confusedly. To prevent this inconveniency, the *malleus* is furnished with three muscles, and the *stapes* with one; by the various contractions of the former the membrane of the *tympanum*, and by means of the latter the membrane of *fenestra ovalis*, is rendered more or less tense, and so accommodated to almost all possible sounds." [46]

Beside this consideration of the muscular reflexes, and of the functions of the sense organs, Whytt is also notable for his contributions to the response view of glandular secretion. [47] He points out that the circulation is important in determining glandular activity, but he also avers that the secretion of tears and of saliva is under nervous control. [48] Perspiration,

[45] *Ibid.*, p. 75.

[46] *Ibid.*, pp. 79 f.

[47] Sherrington (*op. cit.*, p. 786) credits Whytt with the inclusion of salivation under the general heading of reflex action. Whytt, himself, suggests that the idea was not wholly original with him. 'Observations on the Nature, Causes, and Cure of those Disorders which are commonly called Nervous,' p. 515 (note).

[48] *Ibid.*, p. 506 and pp. 515 f. also 'Physiological Essays,' pp. 232 ff. *Cf.* also, Eckhard, *op. cit.*, p. 44.

he contends, is a secretion caused in a similar manner.[49] Moreover, he notes that the condition of the mind affects these responses. "Grief, vexation and fear," he says, "lessen the secretion of saliva."[50] He also notes, in another place, what we should now term, following Pavlov, a 'conditioned salivary reflex.' He points out that even the remembrance of grateful food causes a salivary secretion.[51]

It may be said in this connection that Whytt seems to believe that the ontogenetic process is one of development from certain innate powers into the capacities demonstrated in adult life. Thus in regard to the eye reflex he says, ". . . as the mind has, from the time of birth, been always accustomed to contract the pupils of both eyes at the same time, the one pupil can no more be relaxed without the other being partly relaxed, than one eye can be directed to the nose while the other is turned from it: for how much soever the motions of certain muscles are owing to the immediate energy of the mind, yet it is undeniable, that, by constant habit, we soon lose the power of moving them, except in a particular way. . . ."[52] Again in a statement which seems to contain a certain quaint half-truth for educational psychology he says, ". . . as men by having their ears tied down when young, lose the power of moving them, though there seem to be muscles destined by nature for this purpose. . . ."[53]

In one place Whytt makes a note of a phenomenon which really belongs to that body of observations which are currently brought together under the heading of Weber's Law. He points out that Galen had noted that a greater pain destroys in a considerable degree the feelings of a lesser one. Whytt believed that this observation was confirmed by daily ex-

[49] *Idem*, 'Observations on the Nature, Causes and Cure of those Disorders which are commonly called Nervous,' p. 516.

[50] *Ibid.*, p. 495. This is an example of the modern tone in much of Whytt's work. The present physiological psychology of the external duct glands and of the iris has well been suggested by Cannon, W. B., 'Bodily Changes in Pain, Hunger, Fear and Rage.' *Cf.* esp. pp. 7 *ff.* and p. 24.

[51] *Ibid.*, p. 496. *Cf.*, also, 'Physiological Essays,' p. 231.

[52] *Ibid.*, 'An Essay on the Vital and Other Involuntary Motions of Animals,' pp. 65 *f.*

[53] *Ibid.*, p. 167.

perience. He adds that this finding is analogous to the difference in response given by a patient to a candle when it is brought into a dark room and when it is brought into a chamber flooded by sunlight.[54]

Again, he makes certain observations on phenomena similar to those that in the later work of Bell and Müller became generalized under the heading of the Doctrine of the Specific Energies of Nerves.[55] He says, ". . . that which proves a strong *stimulus* to the nerves of one part will more weakly affect those of another and *vice versa*. . . . Light, which by irritating the *retina* occasions the contraction of the pupil, does not act sensibly as a *stimulus* on any other part of the body." [56] In another place he says, "The nerves of different organs in the same animals are so constituted as to be very differently affected even by the same things: so that we cannot absolutely judge, by our taste or smell, how far any liquor may or may not be adapted to act as a *stimulus* upon the nerves of a particular organ." [57] He also uses the term 'peculiar sensibility' in regard to similar phenomena.[58]

Whytt had observed a phenomenon now sometimes considered under the head of synæsthesia. "The noise of a file and other harsh sounds," he reports, "affect the teeth with an uneasy sensation." [59]

Similarly he had observed a number of abnormal fear reactions, and notably a case of what is now termed 'cat phobia.' He says, "The sight of a cat, nay, even the invisible *effluvia* from that animal, have occasioned anxiety, faintness and sweating." [60] He seems in general to have been interested

[54] *Ibid.*, 'Physiological Essays,' p. 260.

[55] *Cf.* Carmichael, L., *op. cit.*, p. 198 *ff*.

[56] Whytt, 'An Essay on the Vital and Other Involuntary Motions of Animals,' p. 11.

[57] *Ibid.*, p. 28.

[58] *Ibid.*, p. 27. These suggestions of Whytt seem of so definite a nature that it may be that in a previous paper (*op. cit.*) too much assurance was displayed in claiming the absolute priority of discovery of this doctrine for Sir Charles Bell.

[59] *Idem*, 'Observations on the Nature, Cause and Cure of these Disorders commonly called Nervous,' p. 496. For a modern consideration, *Cf.* Titchener, E. B., 'A Text-Book of Psychology,' 1909, pp. 194 *f*.

[60] *Ibid.*, p. 543.

in what he terms the effect of the different passions of the mind upon the body.[61]   He considers the example of blushing in some detail.[62]

Finally, as a practicing psychiatrist his works are indicative of the fact that this branch of the healing art was in his time, as in our own, a most paradoxical subject. "Physicians," Whytt declares, "have bestowed the character of *nervous* on all of those disorders whose nature and causes they were ignorant of." [63]

When writing on functional neurology he seldom seems to forget that his knowledge of the subject was limited and his recommendations for the treatment of the 'mental' disorders of his patients are presented with what would seem to certain modern clinical neurologists a quite unfashionable diffidence.

Whytt frequently prescribed bleeding and opium for what might now be termed a psychological disorder, but at the same time he gave practical common sense directions which may well have proved of value.   He recommends 'soluble-tartar' as a cure for low spirits, but he notes at the same time that exercises and pleasant company should be taken with the prescription.[64]   Moreover he was far from ignorant of some of the principles of modern functional treatment.   "Nervous disorders," he says, "occasioned by strong impressions on the mind are often prevented, lessened, or cured by exciting other sensations or passions of a superior force." [65]

SUMMARY AND CONCLUSION

In this paper the contributions of a Scottish physician, Robert Whytt, to modern physiological psychology have been considered.   His work has, so far as possible, been evaluated in a setting of his own times.   It is shown that he experimentally proved the central nervous system to be essential for neuro-muscular response.   Moreover, he demonstrated that

[61] *Ibid.*, pp. 516 *ff.*

[62] *Loc. cit.   Cf.* also 'Essay on the Vital and other Involuntary Motions,' pp. 55 *f.*

[63] Whytt, 'Observations on the Nature, Causes, and Cure of those Disorders which are commonly called Nervous,' p. 489.

[64] *Ibid.*, p. 712.

[65] *Ibid.*, p. 677.

even a segment of the spinal cord was sufficient to act as a center for a functional reflex arc. His views on the nature of the central nervous system, while not so crude as those of some earlier writers, were, nevertheless, frankly vitalistic. Among his specific contributions to science is a description of the pupillary reflex. This response now bears his name. He demonstrated that glandular action might be brought under the general head of reflex response. He also contributed to the basic lore of inhibition. Finally, brief treatment is given to Whytt's observations on phenomena that would now be considered to fall under the respective headings of Weber's Law, the specific energy of nerves, synæsthesia, and functional nervous disease.

THE INFLUENCE OF EVOLUTIONARY DOCTRINE ON PSYCHOLOGY

BY D. T. HOWARD

Northwestern University

The year 1860 is rightly regarded as momentous in the history of psychology. But amid the constellation of events occurring at that era the publication of Darwin's 'Origin of Species' outshines all others. The theory of evolution made biology a new science, and through biology worked radical transformations in every subject that touches upon life and living things. Psychology came inevitably under the influence of the new way of thinking. Not directly and immediately, as might have been expected, but none the less slowly and surely, evolutionary ideas have produced a complete change of perspective in the science. We ought to speak of psychology as pre-Darwinian and post-Darwinian, since all that is in any essential degree new in the modern viewpoint is the result, directly or indirectly, of Darwinian influences.

The older—pre-Darwinian—psychology was almost universally defined by reference to the physical sciences and particularly to kinetic mechanism. This formulation dates from the beginnings of modern times, when Galileo, Kepler, Descartes and Newton first laid the foundations of the mechanical sciences. The human body, it was thought, must be regarded as physical in the sense in which all material bodies are physical. It came, therefore, under the jurisdiction of physical laws and of physical interpretation. But the soul or mind seemed to lie outside the purely physical plane. For numerous reasons it did not seem to fit into the mechanical scheme. Hence Descartes—and he was by no means alone in the opinion—held that there must be two substances or ultimate kinds of stuff, matter and mind. Thus dualism was created, and in various forms it has persisted down to the present day. A few radicals, like Hobbes, attempted to

develop a purely mechanistic psychology, holding 'matter' alone to be real; a few spiritualists attempted to make 'mind' the ultimate reality; but these proposals did not get very far. The psychology of the men whom we generally think of as the founders of modern (recent) psychology was dualistic, generally parallelistic. In all such systems psychology is aligned with physics, and the 'science of mind' becomes the correlative of the 'science of matter.'

The newer psychology, by contrast, is frankly a biological science. It is aligned with the science of life, rather than with the science of matter. However much it may be argued that such a shift of perspective is superficial, it is unquestionably true that the desertion of the older formulation of the science in favor of evolutionary conceptions resulted in changes of great consequence. Dualism, by way of example, is no longer fashionable. It is not compatible with biological naturalism or at least is extremely difficult to assimilate to the evolutionary point of view.

The fundamental disparity of the older and newer conceptions of psychology is well illustrated in the functional psychology of such writers as Angell and Pillsbury. We must regard these men as transitional thinkers. In the attempt to hold on to the old, while yet advocating the new point of view, they produced systems whose constituent elements failed in logical coherence. Their functionalism, as such, was at war with their dualism: the effort to make psychology at once a 'science of mind' and a 'science of adaptive behavior' could not be carried through. From the one point of view the human individual was to be interpreted as matter and mind, from the other as an integrated whole reacting selectively to the environment; from the one point of view the chief business of the science was to explain the colligation and fusion of mental elements into conscious states, from the other it was to explain the processes by which a living organism maintains its integrity against the forces of external nature.

It may be that in the last analysis this seeming incompatibility of the older and newer conceptions is more apparent

than real. Nobody at any rate has effected a satisfactory synthesis of them. In the effort to escape the confusion of the earlier functionalism, and to arrive at a simpler and more consistent conception of the mental processes, psychologists have tried several devices, among which 'Behaviorism'—the 'ism'—is most conspicuous. There has been, in this country, a pronounced revolt against the older analytical, introspective psychology, with its dualistic background. The revolt has at times been negative in character, and quite undiscriminating. Many have proposed to reject the older psychology *in toto* without due regard either for its historical significance or its positive values. But this will not do. The earlier functionalists were quite correct in believing that evolutionary psychology must be able to utilize the results and methods of the older school. Their failure was due to the wholly natural circumstance that they had not carried the evolutionary point of view through to a consistent conclusion.

The situation has been somewhat complicated, also, by the circumstance that experimental psychology, so indispensable to progress, developed on the older dualistic basis. It is undeniable that in most colleges the psychophysical apparatus of pioneer experimental days gathers dust on laboratory shelves, and it is only here and there that we find a persistent experimentalist still working along the lines initiated by Wundt. The psychology of individual differences has absorbed the attention of many of our younger men, but among those still interested in general psychology not a few are to be found who frankly regard the older experimental methods as antiquated. Here, then, is additional evidence that the revolt against the dualistic position has been more enthusiastic than judicial. How poor a thing the new psychology would be, were it unable even to recognize the unquestionable values of the older laboratory methods! If all the records of psychology, up to 1890, were to be destroyed, the science would go shivering for lack of pin-feathers.

The evolutionary point of view ought to be able, and indeed is able, to avail itself of every good thing that has

emerged from the science since Aristotle. And there have
been many discoveries, many valuable ideas, descriptions,
findings. The Copernican system in astronomy did not over-
throw the Ptolemaic system, root and branch. On the
contrary, it built upon it, and represents a development out
of it. In a similar, if less striking, sense evolutionary psy-
chology is able to utilize the older system and its results in
arriving at a more adequate comprehension of its business as
a science. Evolutionary thought has given to psychology
certain orientating ideas of great potency; but these are as
much in the way of addition as of correction. The new way
of thinking has enriched rather than impoverished the
science.

The nature of the transition from the older to the newer
points of view can be illustrated by reference to the reaction
experiment. Studies of reaction, as everybody knows, began
with the astronomers, passed over into physiology, and were
taken up by Wundt and his school in the early days of experi-
mental psychology. Wundt's psychology was, of course,
dualistic. He studied reaction from its mental rather than
from its physiological side. The experiment provided for
adequate control of physical conditions, after which it became
the business of the psychologist to describe, introspectively,
what goes on in consciousness during the process. Now
study of this kind, using the simple reaction, early revealed
the existence of two 'types' of reaction, sensory and motor.
(For purposes of discussion I avoid complicating the state-
ment.) This was a valid finding, and a valuable one.

Angell and Moore reported, in the PSYCHOLOGICAL RE-
VIEW, in 1896 (Vol. III, pp. 248 f.), a reaction experiment
guided by the new evolutionary conception of the mental
process. These workers quite definitely viewed the reaction
as an adjustment-process, and so viewing it were led to
observe that the 'type' depends upon the direction of the
attention, and *that attention goes to the support of the weakest
spot in the sensory-motor circuit.* Hence the 'type' will vary
according to the individual's needs; if he is uncertain of the
signal, attention will be upon it; if he is uncertain of the

response, then his attention will reënforce the motor side of the circuit. That was a valuable discovery (experiments of my own have confirmed its validity) but one which in no wise relegated the Wundtian findings to the scrap-heap. On the contrary, functionalism here appears as a supplement to the older method.

The most characteristic difference between the two viewpoints, and one that at first sight seems insurmountable, is to be found in the fundamental divergence between their definitions of the mental process. In a dualistic system the mental process occurs in the mind, and consists of a succession of 'mental events' taking place in the mind, parallel with a series of 'bodily events' in the brain. However the dualism be defined, we always have, for practical purposes, the two series of events. This would seem to make it impossible that the mental process should have anything at all to do with the physical process, unless the cumbersome and indirect theory of interaction be assumed. From the functional point of view the mental process is an activity whereby the organism, reacting to stimuli, secures adjustment to its environment. The adaptive response—or, more exactly, the 'preparation' for adaptive response—is an activity of the concrete organism, and is not only concerned with the physiological process going on, but *is* that physiological process in one of its phases. Here there is no dualism of mind and body; the bodily process is mental.

The disparity between these views seemed to certain thinkers so hopeless that they elected to repudiate 'mind' completely and make psychology a science of stimulus and response. Behaviorism is the result. What can the sensations and images of the dualist have to do with the concrete physiological business of adjustment? Curiously enough, these students took a backward step in dealing with dualism. They made psychology a 'science of matter,' while rejecting it as a 'science of mind.' They thus accepted the one side of the dualism while rejecting the other. They should have rejected dualism completely as a formulation of the mind-body relationship. For dualism, as we have said, resulted

from an attempt to align the psychical over against the physical. At heart, it accepts the formulæ of physical science as ultimate for the human mechanism. Evolutionary psychology allies itself with biology, and speaks of organisms rather than of mechanisms. Beginning so, and viewing mind as an adjustment activity of the organism, it has no occasion to postulate dualism. The problem does not directly arise. But there is a point at which it must reckon with the facts which make dualism a plausible theory.

It is necessary to distinguish between mental process and mental content. Perceiving and percept, sensing and sensation, imagining and image, denote on the one side a process and on the other a content. It has already been seen how functionalism defines the mental act. What is it to do with sensations, images, and ideas? Under the dualistic plan these were effects produced in our minds by stimuli acting on the senses or by nerve-currents passing through the cortex. The 'secondary qualities' of the world—colors, sounds, tastes, smells—were thought by early scientists to have no place in the primary world of mass and motion, hence they came to be called 'sensations,' on the theory that they result from sense stimulation, and must therefore be 'inside.' This was of course a purely arbitrary procedure. It made all the secondary qualities—original and 'reproduced'—mental phenomena, mental contents. Evolutionary psychology is not and cannot be committed to that doctrine; it has no place in its scheme of things.

From the functional point of view sensations, percepts, images, are not in themselves 'mental.' No content of experience is by nature, ontologically, a mental fact. These things are not 'in us,' in the sense of being produced in a mind inside us by the action of physical processes; nor are they phenomena 'parallel' thereto, or otherwise detached from a hypothetical physical series of events. The distinction between act and content is a functional, not an existential, distinction. In the newer code the mental process is the organic act of adjustment, the mental 'content' is anything whatever that functions in the way of guidance or orientation

in the course of the act. The act of perceiving is a process of getting adjusted to a situation, and a percept is any object, present to sense, that supplies a cue to effective response. The 'mental' object has meaning or significance in the way of guidance; it is experienced then and there as meaningful. The mental content may be anything whatever that takes on the function of guidance in relation to adjustment. At other times the same thing might exercise no such guidance, and in that case, would be no more mental than a paving stone.

Nevertheless, for the functionalist, sounds, colors, smells, tastes, images do have a significance in relation to adjustment, and he cannot, therefore, fail to be interested in them. He assumes, with certain contemporary dualists, that the world to which the organism has to adjust is the 'world as it is with man left in'; a world of secondary as of primary qualities, a world of feeling and of memory. The ability to discriminate between sensations, to differentiate sharply the pitches, timbres, volumes of sounds, is very important from the viewpoint of organic adjustment. Measures of such capacities make up a large part, for instance, of the Seashore tests of musical ability. Hence all the studies of sensations and images of the older school, all descriptions of feeling and emotion, have their place in the functional scheme of things. Ability to employ these facts of experience as cues to adjustment is a mark of mentality, and the finer the discrimination the higher the mentality.

The behaviorist—of the mechanistic persuasion—has rejected everything but the physicist's world of mass and motion and primary qualities. He must account for the adjustment-process without reference to the secondary qualities which do in fact serve frequently as helps to effective response. How needlessly abstract is his world, how restricted the field to which his explanations are limited! He accepts the evolutionary conception of mental activity, but retrogrades in his treatment of dualism. Let him once make the distinction between the 'mental' and the 'non-mental' in functional terms and he will find it possible to extend the range of his work. He will also find it possible to understand and utilize the results of the older psychology.

Thus by carrying the evolutionary conception through consistently, by giving a functional definition to 'content' as well as to 'act,' the newer psychology finds itself in a position from which it can synthesize and utilize psychological data from all fields, past and present. One more conception, however, so far scarcely touched upon, is imperative. The functionalist must understand what in modern times is meant by an 'organism.' Most emphatically the human body is not a 'mechanism.' The latter belongs to 17th century physics; it is rapidly passing out under the impact of new physical theories. The mistake of thinking of an organism as a mechanism with an attachment, after the fashion of vitalism, is especially to be avoided. An organism is a system of members so constituted that all of its parts are coöperant, all acting together to make up a whole which is a true individual, self-maintaining and self-adjusting. Under this conception 'teleology' takes on a new meaning; the 'purposiveness' of minds becomes a concrete and describable fact. Thus psychology finds itself freed from that hampering form of metaphysics, mechanical determinism, which has lately caught the public ear by claiming to be 'science,' and is able to go about its job of analyzing, describing, and measuring mental processes with a minimum of interference. The 'organismic' idea, fostered by Ward, Dewey, Kantor, and the 'Gestalt' school, is making rapid progress in psychology. This is natural, since it is kin of the evolutionary concepts which have worked such great changes in the science in the past thirty years.

AN INTERPRETATION OF THE WEBER-FECHNER LAW

BY HARVEY CARR

The University of Chicago

The Weber-Fechner law attempts to state in quantitative terms the disproportional relation that exists between increments of the material stimulus and the corresponding increments of sensory effect.

Two distinct facts are involved in this relationship. (1) As the stimulus is progressively increased by equal increments of an effective magnitude, the corresponding increments of sensory effect keep decreasing. By an effective increment we mean one that does produce an effect that can be consistently noted. (2) All minimal increments of the stimulus may produce no *apparent* sensory effect, *i.e.*, an effect that can be consistently noticed in 75 per cent of the observations.

The first fact that equal increments of the stimulus do not give equal increments of sensory effect is, however, but a special case of a much more general phenomenon, viz., the similar disproportionality that frequently exists between measured increments of cause and the corresponding increments of effect. For example, in the typical form of the learning curve equal increments of measured practice do not give equal increments of measured improvement. The law of diminishing returns is a pertinent illustration in the field of economics. In the physical realm, illustrations are numerous. Equal increments of gas do not give equal increments of speed, equal increments of water pressure do not give equal increments in rate of flow, and equal increments of electric current do not give equal increments of light. The first aspect of the psychophysical relation is thus not peculiar to the sensory domain, nor even to the realm of mind; it is but a special case of a phenomenon that is also found in many cause and effect relations.

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If this feature of the psychophysical relation is but a special case of a more general phenomenon, our explanation of it should, presumably, be couched in terms of principles and concepts that are also applicable to the whole range of situations in which it is found.

As a matter of fact, the conventional explanations of the psychophysical relation, with the exception of those of Fechner and of Wundt, do fulfill the above criterion. In our practical measurements, we usually do not measure the *total effect* of any causal condition. In the transformation of energy, resistance is encountered, and much energy is necessarily dissipated into other channels and contributes nothing to the particular effect in which we are interested and which we are measuring. For example, much of the energy of the gas is transformed into heat and contributes nothing to speed, and not all of the energy of the electric current is transformed into light. As the causal energy is gradually increased, it is assumed that relatively more and more resistance is encountered, relatively more and more energy is dissipated into other channels, and that as a consequence the corresponding increments of effect become less and less. Likewise in the psychophysical relation, a certain amount of energy is dissipated in the process of transmission and conduction, and contributes nothing to the sensory effect. As the stimulus is increased, the resistance encountered becomes relatively greater, and the increments of sensory effect become less and less.

Theories that adopt this type of explanation differ primarily in respect to the *locus* of this loss of energy. There is no good reason, however, for assuming that this loss of energy occurs exclusively at any particular place. A certain amount may be lost in overcoming the resistance in any of the conducting fibers, some may be drawn off into reflex channels at the lower centers, a certain amount may be transformed into other forms of energy in the receptive structures, and theoretically a slight amount may be dissipated along the route between the sense organ and the stimulating object.

We are willing to admit that the *first feature* of the psycho-physical relation can best be explained in terms of the concept of increasing resistance and the consequent dissipation of energy into other channels. The *second characteristic* of this relation, in my opinion, cannot be readily accounted for in such terms, and we wish to suggest a more feasible explanation for this particular fact.

Neither is this second characteristic peculiar to the psycho-physical relation, and a few illustrations from the physical realm will serve to suggest the type of explanation to be proposed.

We shall first resort to a crude mechanical illustration. Two shafts project from either end of a metal box. On one end of each shaft is attached a finely calibrated dial, and within the box the two shafts are so connected by means of gears that a rotation of one dial will produce an equal movement on the part of the second dial. The two dials are also alike in respect to size and calibration. It is obvious that this device will exhibit neither feature of the psychophysical relation, for any measurable rotation of one dial will produce an equal measurable movement on the part of the other.

Let us now assume that the two shafts are geared together in the ratio of twenty to one. It is obvious that this mechanical device will exhibit the second aspect of the psychophysical relation, for each minimal rotation of dial *A* will exert no measurable influence upon dial *B* because the two units of measurement are not commensurate in respect to the magnitudes compared.

We may also assume that the two shafts are geared in the ratio of one to one, but that the two dials differ considerably in circumference, or that the rotation of the first shaft is controlled and measured by means of a finely calibrated screw micrometer. This device will again exhibit the second characteristic of the psychophysical relation in virtue of the fact that the two measuring devices differ in respect to the fineness or delicacy of the quantitative distinctions that they mediate. The two units of measurement are not equivalent magnitudes.

It is also possible to connect the two shafts by mechanical means in such a manner that a series of equal increments of movement on the part of dial *A* will produce a corresponding series of decreasing increments on the part of dial *B*. Obviously such a device will exhibit *both aspects* of the psychophysical relation.

A volume of water is heated by electrical means. Let us assume that the current is measured by an extremely delicate device, while the heat of the water is measured by a crude thermometer which gives no finer readings than one degree Centigrade. It will now be necessary to increase the current by a considerable number of measurable increments before any corresponding heat effect can be detected with the measuring device employed. Given a thermometer that reads accurately to one tenth of a degree, and the discrepancy between the two sets of readings will be correspondingly lessened. Given a finer and more sensitive thermometer, and it is theoretically possible that the discrepancy, at least, within certain intensity limits, will altogether disappear, and that within these limits each measurable increment of current will exert a measurable effect on the temperature of the water. Obviously this cause and effect situation may or may not exhibit the second characteristic of the psychophysical relation, and its absence or presence is a function of the equivalence or non-equivalence of the two units of measurement employed.

Science is continually attempting to devise more and more delicate devices for measurement, to measure in smaller and smaller units, but the delicacy of measurement attained necessarily varies with the nature of that which we are measuring. In all cause and effect relations that involve a transformation of energy from one form to another, as the change of electrical energy into heat or light, it is at once obvious that the two units employed will rarely, if ever, happen to be equivalent magnitudes. Because of this non-equivalence of the units of measurement, we may sometimes find that many increments of cause produce no measurable effect, while *conversely* for other situations we may be able to

observe measurable variations of effect for which no corresponding fluctuations in the causal conditions can be detected. In both cases the discrepancy between the two sets of measurements is merely due to the inadequacy of one of our means of measurement.

This concept of the non-equivalence of the two units of measurement will thus account for the second aspect of the psychophysical relation. In the psychophysical realm this non-equivalence of the units may be due to several factors.

For the most part, the non-equivalence is a function of the means of measurement. The differences in sensory effect are directly measured by an *unaided* sense organ, while the corresponding variations of the material stimulus are measured with the aid of physical principles and mechanical devices that enable the sensitive structures involved to detect changes that would otherwise escape notice. The same sense organ, *e.g.*, the eye, may be employed in both measurements, but in one case its sensitivity is greatly enhanced by mechanical aids, and in the other it is not. With the aid of such measuring devices, necessarily we can detect many changes in the stimulating conditions for which we can detect no corresponding sensory effect.

All measurements of the kind with which we are here concerned necessarily involve a sensitive structure. The units of measurement must be perceptible magnitudes, and it is at once obvious that the fineness or delicacy of the units that can be employed is a function of the discriminative capacity of the sense organ involved. In the physical realm, practically all measurements are made in terms of the *same sense*, viz., vision. In the psychophysical realm, the situation is necessarily different. For all senses other than vision, the increments of the material stimulus are usually measured in terms of the eye, while the corresponding sensory effect is apprehended through some other sensory avenue. Inasmuch as we are here dealing with *minimal* units of magnitude, and inasmuch as the size of these units is a function of the discriminative capacity of the sense organs concerned, it is at once obvious that the two units of measurement are not

likely to be equivalent magnitudes when two *different sense organs* are involved. In other words, one has no right to assume that the discriminative thresholds of different sense organs are equivalent magnitudes.
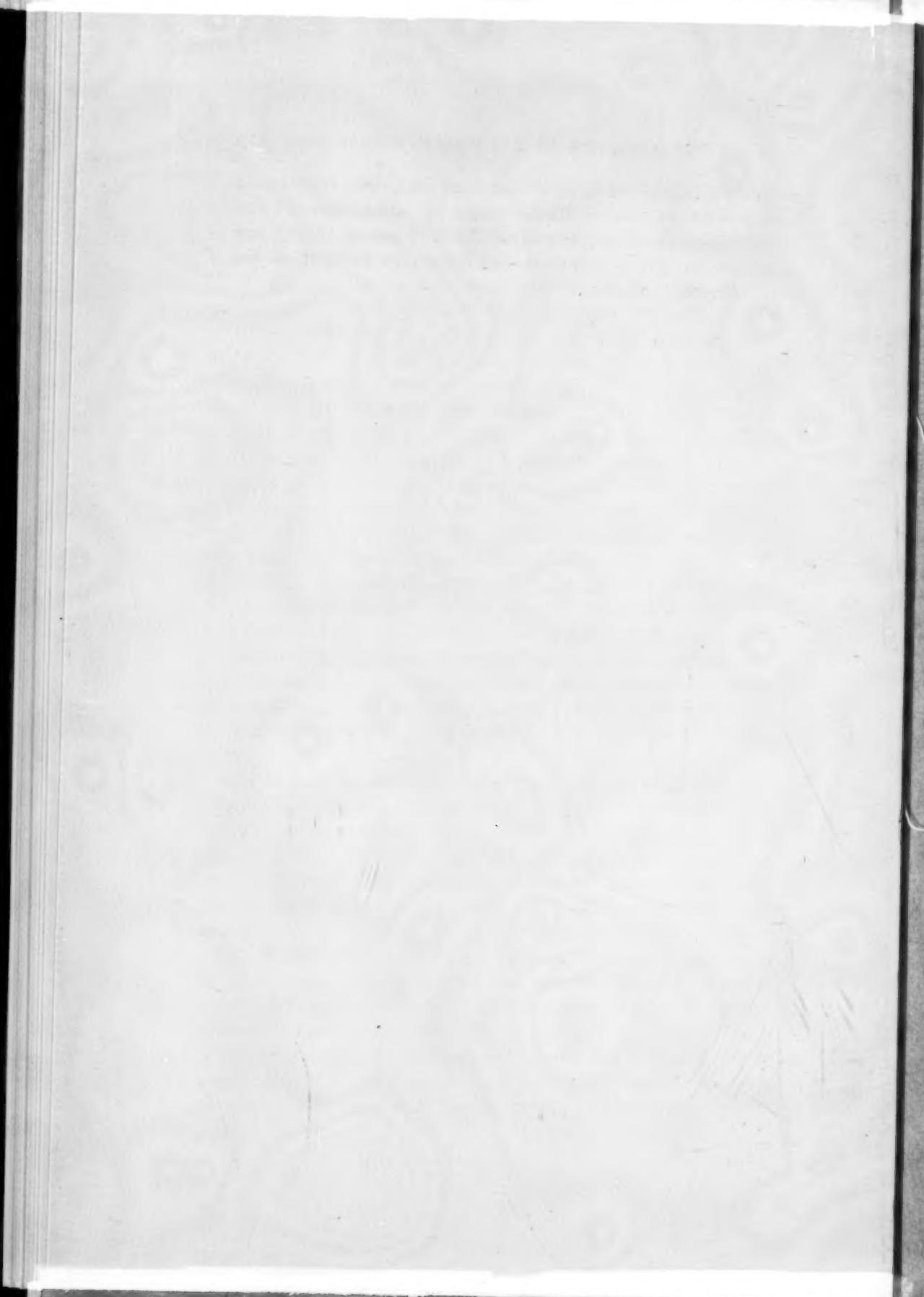
A third factor is somewhat similar to the second. In measuring the increments of the material stimulus, the size of the unit is usually a function of the *spatial* sensitivity of the eye, while the unit of sensory effect, in the typical form of the psychophysical experiment, is a function of the *intensity* sensitivity of the organ concerned. Even though the eye may be involved in both series of measurements, we cannot assume that it is equally sensitive in these two diverse respects, or that the two units of measurement are equivalent magnitudes. Again, one has no right to assume that the spatial and intensity discriminative thresholds of any sense organ are equivalent magnitudes.

Finally, the non-equivalence of the units is a necessary consequent of the first factual characteristic of the psycho-physical relation—the fact that the number of stimulus units corresponding to a unit of sensory effect keeps increasing as we ascend the intensity scale. If the two units are equivalent magnitudes in any one part of the scale, it is obvious that they cannot also be equivalent for any other part of that scale.

According to our conception, every increment of stimulus theoretically produces some sensory effect. The two series are in reality continuous variables, and the apparent or observed discrepancy between the two is due to the fact that we can make finer or rather *more numerous* distinctions within the one than we can in the other. The units of measurement are thus not comparable or equivalent magnitudes. The conception is thus essentially similar to that of Wundt who assumed that every increment of stimulus does produce a sensory effect, but that these effects cannot always be apper-ceived, or cognitively identified.

In conclusion, two distinct phenomena are involved in the psychophysical relation, and neither is peculiar to that relation. The concept of resistance and dissipation of energy

is nicely adapted to explain the first fact, but the second fact is best accounted for in terms of some form of the Wundtian type of explanation. Both types of theory are necessary to give a complete and adequate account of the psychophysical relation.

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